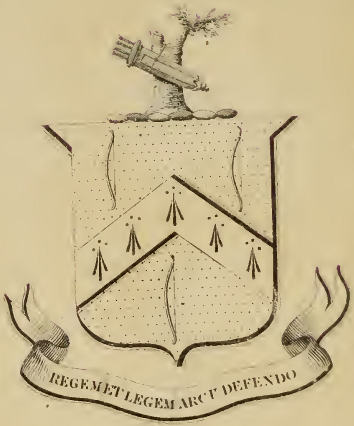

R. B. Bowman.

At a Meeting of the Coun-
cil of the Royal Society,
Feb. 28, 173$\frac{2}{3}$.

THE greateſt Part of this Second Volume of Statical ESSAYS, *by* Stephen Hales, B. D. F. R. S. *having been read at ſeveral Meetings of the Royal Society, for which he had their Thanks, it is ordered that he be deſired to Print the ſame.*

Hans Sloane, P. R. S.

Physical

Statical ESSAYS:

CONTAINING

HÆMASTATICKS;

OR,

An Account of ſome HYDRAULICK and HYDROSTATICAL Experiments made on the Blood and Blood-Veſſels of Animals.

ALSO

An Account of ſome Experiments on Stones in the Kidneys and Bladder; with an Enquiry into the Nature of thoſe anomalous Concretions.

To which is added,

An *APPENDIX*,

CONTAINING

OBSERVATIONS and EXPERIMENTS relating to ſeveral Subjects in the firſt Volume. The greateſt Part of which were read at ſeveral Meetings before the Royal Society.

With an INDEX to both VOLUMES.

VOL. II.

Deſideratur Philoſophia Naturalis vera & activa cui Medicinæ Scientia inedificetur.

Fran. de Verul. Inſtaur. Magna.

By *STEPHEN HALES*, B. D. F. R. S.
Rector of *Farringdon*, *Hampſhire*, and
Miniſter of *Teddington*, *Middleſex*.

LONDON:
Printed for W. INNYS and R. MANBY, at the Weſt-End of St. *Paul's*;
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TO THE

KING's

Most Excellent Majesty.

SIR,

YOUR Majesty's gracious Acceptance of my former Volume of Experiments, has encouraged me, both further to pursue these natural Researches, and also

to lay the Refult of them at Your Feet.

The Study of Nature will ever yield us frefh Matter of Entertainment, and we have great reafon to blefs God, for the Faculties and Abilities he has given us, and the ftrong Defire he has implanted in our Minds, to fearch into and contemplate his Works, in which the farther we go, the more we fee the Signatures of his Wifdom and Power, every Thing pleafes and inftructs us, becaufe in every Thing we fee a wife Defign.

As the beautiful Fabrick of this World was chiefly framed for and adapted to the Uſe of Man, ſo the greater inſight we get into the Nature and Properties of Things, ſo much the more Beneficial will they be to us, the more will our real Riches thereby increaſe, the more alſo will Man's original Grant of Dominion over the Creatures be inlarged.

Your Majeſty's Subjects of *Great Britain* are allowed by the candid Confeſſion of other Nations, to excell in experimental Philoſophy, which has

long been found to be moſt beneficial to Mankind.

As the Advancement of Arts and Sciences much depends on the Protection of Princes, whoſe Patronage they are well worthy of; ſo we have a pleaſing Proſpect of their flouriſhing under Your Majeſty's auſpicious Favour, whoſe Care and Concern for the Welfare and Proſperity of His People, is in every reſpect moſt extenſive.

That Your Majeſty, after having long continued a

Bleſſing

Bleſſing to Your Subjects in a proſperous Reign here on Earth, may hereafter enjoy a happy Immortality in Heaven, is the ſincere Prayer of,

May it pleaſe Your Majeſty,

Your Majeſty's

moſt humble and

dutiful Subject,

STEPHEN HALES.

THE

PREFACE.

WHAT I had at firſt intended only as additional Obſervations and Experiments to the firſt Volume, is now grown into the Size of another Volume, ſo fruitful are the Works of the great Author of Nature in rewarding by farther Diſcoveries, the Reſearches of thoſe *who have Pleaſure therein*: We can never indeed want Matter for new Experiments; and tho' the Hiſtory of Nature as recorded from almoſt innumerable Experiments, which have been made within the compaſs of a Century, be very large, yet the Properties of Bodies are ſo various, and the different Ways by which they may be examined

examined ſo infinite, that 'tis no wonder that we are as yet got little farther than to the Surface of Things: Yet ought we not to be diſcouraged, for tho' we can never hope to attain to the compleat Knowledge of the Texture, or conſtituent Frame and Nature of Bodies, yet may we reaſonably expect by this Method of Experiments, to make farther and farther Advances abundantly ſufficient to reward our Pains.

And tho' this Method be tedious, yet our Abilities can proceed no faſter; for as the learned Author of the *Procedure of human Underſtanding* obſerves, *pag.* 205, 206. " All the real true Knowledge we " have of Nature is intirely *experi-* " *mental*, inſomuch that, how " ſtrange ſoever the Aſſertion ſeems, " we may lay this down as the firſt

 " fundamental

" fundamental unerring Rule in " Physicks, *That it is not within* " *the compass of human Understand-* " *ing to assign a purely speculative* " *Reason for any one Phænomenon* " *in Nature.*" So that in natural Philosophy, we cannot depend on any meer Speculations of the Mind; we can only with the Mathematicians, reason with any tolerable Certainty from proper *Data*, such as arise from the united Testimony of many good and credible Experiments.

Yet it seems not unreasonable on the other hand, tho' not far to indulge, yet to carry our Reasonings a little farther than the plain Evidence of Experiments will warrant; for since at the utmost Boundaries of those Things which we clearly know, there is a kind of Twilight cast

caſt from what we know, on the adjoining Borders of *Terra incognita*, it ſeems therefore reaſonable in ſome degree to indulge Conjecture there; otherwiſe we ſhould make but very ſlow Advances in future Diſcoveries, either by Experiments or Reaſoning: For new Experiments and Diſcoveries do uſually owe their firſt Riſe only to lucky Gueſſes and probable Conjectures, and even Diſappointments in theſe Conjectures, do often lead to the Thing ſought for: Thus by obſerving the Errors and Defects of a firſt Experiment in any Reſearches, we are ſometimes carried to ſuch fundamental Experiments, as lead to a large Series of many other uſeful Experiments and important Diſcoveries.

If therefore ſome may be apt to think that I have ſometimes too

far

far indulged Conjecture, in the Inferences I have drawn from the Events of ſome Experiments; they ought to conſider that it is from theſe kind of Conjectures that freſh Diſcoveries firſt take their Riſe; for tho' ſome of them may prove falſe, yet they often lead to further and new Diſcoveries. It is by the like Conjectures that I have been led on, Step by Step, thro' this long and laborious Series of Experiments; in any of which I did not certainly know what the Event would be, till I had made the Trial, which Trial often led on to more Conjectures and farther Experiments.

In which Method we may be continually making farther and farther Advances in the Knowledge of Nature, in proportion to the Number of Obſervations which we have: But

But as we can never hope to be furnished with a sufficient Number of these, to let us into a thorough knowledge of the great and intricate Scheme of Nature, so it would be but dry Work to be ever laying Foundations, but never attempting to build on them. We must be content in this our infant State of Knowledge, while we *know in part* only, to imitate Children, who for want of better Skill and Abilities, and of more proper Materials, amuse themselves with slight Buildings. The farther Advances we make in the Knowledge of Nature, the more probable and the nearer to Truth will our Conjectures approach: so that succeeding Generations, who shall have the Benefit and Advantage both of their own Observations, and those of preceding Generations, may

then

then make confiderable Advances, when *many ſhall run to and fro, and Knowledge ſhall be increaſed,* Dan. xii. 4. In the mean time, it would but ill become us in this our State of Uncertainty, to treat the Errors and Miſtakes of others with Scorn and Contempt, when we cannot but be conſcious, that we our ſelves ſee Things *but as thro' a Glaſs darkly*, and are very far from any Pretenſions to Infallibility.

As it has ever been of great Importance to the Welfare of Mankind, to make the beſt Reſearches they can into the Nature of our Bodies, ſo have many eminent Perſons, from time to time, made conſiderable Diſcoveries therein. And as the animal Fluids move by Hydraulick and Hydroſtatical Laws, ſo I have here made ſome enquiry into the Nature of their

Motions by a ſuitable Series of Experiments. The diſagreeableneſs of the Work did long diſcourage me from engaging in it; but I was on the other hand ſpurred on by the hopes that we might thereby get ſome farther Inſight into the animal OEconomy. We find here a large Field for Experiments, which may be multiplied and varied many Ways, of which I have here only given a few Specimens. As theſe Experiments do obviouſly and clearly give an Account of ſome *Phænomena*, ſo they may poſſibly be of Service in the Hands of thoſe who are well skilled in the animal OEconomy and the Hiſtory of Diſeaſes, to explain many other of the innumerable Variety of Caſes, which occur in ſo complicated a Subject as an animal Body is.

In

In which we are aſſured that all Things are wiſely adjuſted in Number, Weight and Meaſure, yet with ſuch complex Circumſtances as require many *Data* from Experiments, whereon to found juſt Calculations: But tho' many of the following Calculations are founded only on ſuch inaccurate Menſurations as the Nature of the Subject would allow of, yet may we thence fairly draw many rational Deductions in relation to the animal OEconomy.

In which there is ſo juſt a Symmetry of Parts, ſuch innumerable Beauties and Harmony in the uniform Frame and Texture of ſo vaſt a Variety of ſolid and fluid Parts, as muſt ever afford Room for farther Diſcoveries to the diligent Enquirer; and thereby yield freſh Inſtances to illuſtrate the Wiſdom of the divine

Architect, the Traces of which are ſo plain to be ſeen in every Thing, that the *Pſalmiſt* had good reaſon to call him a *Fool* who could be ſo ſenſeleſs as to *ſay in his Heart that there is no God*; whoſe maſterly Hand is ſo evident in every Part of Nature, that if there be any who pretend they cannot ſee it, it can be no Breach of Charity to ſay that they are wilfully blind, and therefore Lyars.

In the Treatiſe on the *Calculus*, I have endeavoured by a great Variety of Experiments, to enquire into the Nature of that moſt formidable Concretion; but tho' I have not thence been able, to find out either a ſure Preventive, or a ſafe Diſſolvent, yet I am not without hopes that theſe Reſearches, as they were ſincerely intended, ſo they will prove of conſiderable Benefit to Mankind in many

Caſes,

Cafes, by fhewing not only the Nature, but alfo the Caufes which are moft apt, either to promote or retard the Progrefs of this Secret and terrible Intruder; not that it is to be expected, that all fhould find Benefit thereby, tho' haply fome may. The Inftrument defcribed at the End of this Treatife, will, I doubt not, be very beneficial in inftantly relieving many, who might otherwife fuffer great Torture for feveral Days, and fome of them lofe their Lives by a dangerous Incifion.

The Appendix contains feveral mifcellaneous Obfervations and Experiments, fome of which relate to Subjects in *Vegetable Staticks*, but the greateft Part to the *Analyfis* of the Air in the firft Volume.

I have added a general Index of the Matters contained in thefe two

Volumes. But as this Index is adapted to the ſecond Edition of the firſt Volume; ſo there muſt be an Allowance for a difference of eight Pages leſs, from Page 75 of the firſt Edition of the firſt Volume; which difference in the numbering of the Pages of the two Editions of the firſt Volume, continually decreaſes, ſo as to be none in the laſt Page of each Edition.

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THE
INTRODUCTION.

1. AS an animal Body conſiſts not only of a wonderful texture of ſolid Parts, but alſo of a large proportion of Fluids, which are continually circulating and flowing, thro' an inimitable Embroidery of Blood-Veſſels, and other inconceivably minute Canals: And as the healthy State of an Animal principally conſiſts, in the maintaining of a due *Equilibrium* between thoſe Solids and Fluids; it has, ever ſince the important Diſcovery of the Circulation of the Blood, been looked upon as a Matter well worth the enquiring into, to find the Force and Velocity with which theſe Fluids are impelled; as a likely means to give a conſiderable Inſight into the animal OEconomy.

2. Several ingenious Perſons have from time to time, attempted to make Eſtimates of the Force of the Blood in the Heart and Arteries, who have as widely differed from each other as they have from the Truth, for want of a ſufficient Number of *Data* to argue from: Had Perſons of their Abilities been

been more careful, in the firſt Place to get what Inſight they could into the Matter, as far as a regular Series of proper Experiments would have informed them, they would then doubtleſs have been furniſhed, with more and more proper *Data* whereon to found their Calculations, which would have brought them much nearer to the Truth.

3. Finding therefore but little Satisfaction, in what had been attempted on this Subject by *Borellus* and others, I endeavoured about twenty-five Years ſince by proper Experiments, to find what was the real Force of the Blood in the crural Arteries of Dogs, and about ſix Years afterwards I repeated the like Experiments on two Horſes, and a fallow Doe; but did then not purſue the Matter any further, being diſcouraged by the diſagreeableneſs of anatomical Diſſections. But having of late Years found by Experience the Advantage of making uſe of the Statical Way of Inveſtigation, not only in our Reſearches into the Nature of Vegetables, but alſo in the chymical Analyſis of the Air; I was hence induced to hope for ſome Succeſs, if the ſame Method of Enquiry were applied to animal

Bodies;

Bodies; especially considering that they are in a manner framed of one continued Maze of innumerable Canals, in which Fluids are incessantly circulating, some with great Force and Rapidity, others with very different Degrees of rebated Velocity: Hence I was encouraged to resume the Enquiry, by Variety of such Experiments as I conjectured would give some Light into the Matter.

4. It may well be wondered at, that one who neither by Profession nor Inclination, is prompted to deal in anatomical Operations, should engage in a Thing of this Nature; especially in an Age and Nation that abounds with ingenious Persons, who are eminently skilled in Anatomy; and who have brought the Art of preparing and injecting the finer capillary Vessels, with proper coloured Ingredients, to a good Degree of Perfection.

5. But as the animal Canals have not, that I know of, been hitherto examined any other Way, than by the imperfect and uncertain Methods of injecting with a Syringe and blowing into them! I hope it will appear from this Specimen to be worth the while to attempt it in the following

Hydraulick

Hydraulick Way, which ſeems to me to be more accurate and certain. And I hope that what I have here done in this Way will induce more skilful Anatomiſts to purſue the Matter farther by a great Variety of the like Experiments on different Parts of the Body, and alſo with Variety of Liquors, of very different Degrees of Conſiſtency, as to their Viſcidity or Thinneſs and Fluidity, Reſtringency and other Qualities; whence I make no doubt but many curious Obſervations and Diſcoveries would ariſe. For ſince we are aſſured that the animal Fluids move by *Hydraulick* and *Hydroſtatick* Laws, the likelieſt Way therefore to ſucceed in our Enquiries into the Nature of their Motions, is by adapting our Experiments to thoſe Laws.

6. So complicated and curiouſly wrought a Fabrick as an animal Body is, the admirable and amazing Texture of every Part of which, declares its divine Original, and whoſe Welfare and good Order conſiſting in the concurring Harmony of ſuch innumerable Circumſtances, will, in whatever View we conſider it, ever afford freſh Matter to reward our Reſearches.

AN ACCOUNT OF SOME Hydraulick and Hydroſtatical EXPERIMENTS made on the Blood and Blood-Veſſels of ANIMALS.

EXPERIMENT I.

1. IN *December* I cauſed a Mare to be tied down alive on her Back, ſhe was fourteen Hands high, and about fourteen Years of Age, had a *Fiſtula* on her Withers, was neither very lean, nor yet luſty: Having laid open the left crural Artery about three Inches from her Belly, I inſerted into it a braſs Pipe whoſe Bore was one ſixth of an Inch in Diameter; and to that, by means of another braſs Pipe which was fitly adapted to it, I fixed a glaſs Tube, of nearly the ſame Diameter, which was nine Feet in Length: Then untying the Ligature on the Artery, the Blood roſe in the

Tube eight Feet three Inches perpendicular above the Level of the left Ventricle of the Heart: But it did not attain to its full Height at once; it ruſhed up about half way in an Inſtant, and afterwards gradually at each Pulſe twelve, eight, ſix, four, two, and ſometimes one Inch: When it was at its full Height, it would riſe and fall at and after each Pulſe two, three, or four Inches; and ſometimes it would fall twelve or fourteen Inches, and have there for a time the ſame Vibrations up and down at and after each Pulſe, as it had, when it was at its full Height; to which it would riſe again, after forty or fifty Pulſes.

2. The Pulſe of a Horſe that is well, and not terrified, nor in any Pain, is about thirty ſix Beats in a Minute, which is nearly half as faſt as the Pulſe of a Man in Health: This Mare's Pulſe beat about fifty five times in a Minute, and ſometimes ſixty or a hundred ſhe being in pain.

3. Then I took away the glaſs Tube and let the Blood from the Artery mount up in the open Air, when the greateſt Height of its Jet was not above two Feet.

4. I meaſured the Blood as it run out of the Artery, and after each Quart of Blood was run out, I refixed the glaſs Tube to the Artery

Artery, to ſee how much the Force of the Blood was abated ; this I repeated to the eighth Quart, and then its Force being much abated, I applied the glaſs Tube after each Pint had flowed out: The Reſult of each Trial was as is ſet down in the following Table, in which are noted the greateſt Heights it reached after every Evacuation: It was uſually about a Minute before it roſe to theſe ſeveral Heights, and did not riſe gradually, but would ſtand during ſeveral Pulſes much lower, than what it would at length reach to; ſo that I often thought it had done riſing, when on a ſudden it would riſe for ſome time four, eight, twelve or ſixteen Inches higher, where it would ſtay for ſome time, and then on a ſudden fall four, eight, twelve or ſixteen Inches.

The several Trials.	The Quantities of Blood let out in Wine Measure.		The several Heights of the Blood after these evacuations	
	Quarts	Pints	Feet	Inches
1	0	* 5 Ounces	8	3
2	1	0	7	8
3	2		7	2
4	3		6	6 ½
5	4		6	10 ½
6	5		6	½
7	6		5	5 ½
8	7		4	8
9	8		3	3
10	8	1	3	7 ½
11	9	0	3	10
12	9	1	3	6 ½
13	10	0	3	9 ½
14	10	1	4	3 ½
15	11	0	3	8
16	11	1	3	10 ½
17	12	0	3	9
18	12	1	3	7 ½
19	13	0	3	2
20	13	1	4	½
21	14	0	3	9
22	14	1	3	3
23	15	0	3	4 ½
24	15	1	3	1
25	16	0	2	4

* These 5 Ounces lost in preparing the Artery.

By this time there is a Pint lost in making the several Trials, which is not allowed for in this Table.

There was about a Quart lost in making the several Trials, so there flowed out in all seventeen Quarts, and half a Pint after the last Trial, when she expired. This whole Quantity of Blood was equal to 1185.3 cubick Inches.

5. We may observe from this Table, that the Decrease of the Force of the Blood in the Arteries, was not proportioned to the several Quantities of Blood which were evacuated; for at the eighth Trial, when seven Quarts were drawn off, the Height of the Blood was four Feet eight Inches, after which it decreased in the five following Trials to three Feet odd Inches, sometimes a little lower and then a few Inches higher. But at the fourteenth Trial, after ten Quarts and a Pint had been drawn off, it rose again up to four Feet $3 + \frac{1}{2}$ Inches, and it came nearly to the same Height again at the twentieth Trial when thirteen Quarts and a Pint had been drawn off.

6. This disproportionate Inequality in the several Heights was principally owing to her violent straining to get loose, which made the Blood in the fourteenth Trial rise higher than it had done in several of the preceding ones.

7. About the twentieth Trial she grew very faint and uneasy, and breathed quick: The violent straining to get loose, did by the acting of most of her Muscles, especially the abdominal, impell the Blood from all Parts to the *Vena Cava*, and consequently there

was a greater Supply for the Heart, which muſt therefore throw out more at each Pulſation, and thereby increaſe the Force of the Blood in the Arteries.

8. For the ſame Reaſon too, it would be ſomewhat increaſed in Height upon deep ſighing, becauſe the Lungs being then put into greater Motion and more dilated, the Blood paſſed more freely and in greater Quantity, to the left Auricle and thence to the Ventricle.

9. This plainly ſhews how ſighing increaſes the Force of the Blood, and conſequently proportionably chears and relieves Nature, when oppreſſed by its too ſlow Motion, which is the Caſe of thoſe who are dejected and ſad.

10. Hence alſo we ſee evidently, that the Blood moves faſteſt and moſt freely thro' the Lungs when they are in a dilated State: For which Reaſon Animals when they are near expiring, do uſually breathe quick, the Lungs then labouring to heave faſt, that the languid Blood may thereby have a freer Courſe thro' them, to ſupply the then almoſt bloodleſs Pulſations of the Heart, as was we ſee the Caſe of this Mare when her Blood was near exhauſted.

11. When between fourteen and fifteen Quarts of Blood had been evacuated, and thereby the Force of that which remained in the Veſſels greatly decreaſed, then the Mare fell into cold clammy Sweats, ſuch as frequently attend dying Perſons; which ſhews to how low a State the vital Force of the Blood is at that Time reduced: Whence we ſee, that theſe faint Sweats are not occaſioned by a greater protruſive Force of the Blood at that Time, but rather by a general Relaxation of the Pores, as well as of all other Parts of the Body. And it ſeems hence probable, that the Vigour of the Blood in the Arteries is much abated, when Perſons who are not in a dying State, have colliquative Sweats, as in violent Colic Pains, Fear, *&c.*

12. Upon opening the Mare's Body, I found little or no Blood in the *Aorta*, about an Ounce in the left Ventricle, but none in the right; the *Vena Porta* and *Cava* were full: She bled two or three Ounces, but very ſlowly, and not without preſſing the jugular Vein, which was opened as ſoon as ſhe expired.

13. There might be about two Quarts and three Quarters of Blood left in the large

Veins, which, with what was drawn out at the Artery, makes up twenty Quarts, equal to 1154 cubick Inches, or forty four Pounds; which at a low Eſtimation, may be reckoned the Quantity of current Blood in a Horſe; there is doubtleſs conſiderably more, but it is not eaſy to determine how much.

14. As this Experiment ſhews how much the Force of the Blood in the Arteries is abated by different Degrees of Evacuation; ſo it may be of uſe to direct what Quantity to let out at a Time in bleeding: For whatever the real Quantity of the circulating Blood be, it is certain that the Eſtimate of what can be with ſafety let out at once, muſt be taken from the Proportion which that bears to the whole Quantity of Blood, which will flow out of the Vein or Artery of the Animal till it dies.

15. We ſee alſo from this Experiment, the Reaſonableneſs of the Practice of bleeding at ſeveral diſtant Times, where it is requiſite to take away a great Quantity of Blood, and not to do it all at once, which would too much weaken the Force of the Blood. For ſince it was found by ſeveral Inſtances in this Experiment, that when the Force of the Blood

Blood was much depreſſed by Evacuations, it would be conſiderably raiſed again by the Action of the Muſcles, out of whoſe very fine and long capillary Veſſels it moves but ſlowly, as alſo by the Motion of all Parts of the Mare; ſo the Caſe is doubtleſs the ſame, when the Vigour of the Blood is in any Degree rebated in the large Veſſels, by Blood-letting, that Vigour will in ſome meaſure be in a little Time reſtored again, not only by the Action of the ſeveral Parts of his Body, whereby the Blood would have time to flow in from all Parts, to ſupply the moſt evacuated Veſſels, whereby there would be a juſt proportionate Evacuation of all Parts; but alſo becauſe the Veſſels themſelves would thereby have time to contract themſelves in ſome Proportion to the Degree of their Evacuation.

EXPERIMENT II.

1. IN *January* I cauſed a Gelding to be tied down faſt on his Back, in the ſame manner as the Mare was in the foregoing Experiment: He was thirteen Hands high, and ten or eleven Years old, but very lame, by reaſon of

a Canker

a Canker in his Hoof; he was lean, but ſomewhat luſtier than the Mare, and much more lively. I fixed the ſame Braſs Pipe and Glaſs Tube, as above, to his left crural Artery.

2. The Blood ruſhed up the Tube at once, to near two thirds of its greateſt Height, and then more leiſurely as in the Mare. It would riſe and fall commonly about an Inch at each Pulſation of the Heart, but ſometimes two or three Inches. I let out the Blood gradually as in the Mare, and after each Evacuation, I refixed the Glaſs Tube to the Artery, to take the ſeveral Heights of the Blood: The Reſult of each Trial was as noted in the following Table.

3. When I firſt fixed the Tube to the Artery, I ſtopped the Horſe's Noſtrils, ſo as to make him breathe with great Difficulty, which made the Blood riſe five Inches higher; but I could not carry this Experiment almoſt to Suffocation, as I would have done, becauſe his plunging obliged me to take the Tube from the Artery.

The

The ſeveral Trials.	The Quantity of Blood let out.		The ſeveral Heights of the Blood after thoſe Evacuations.		
	Quarts.	Pints.	Feet.	Inches.	
1	0	1	9	8	
2	1		9	8	
3	2		9	5½	
4	3		8	4	
5	4		8	2	
6	5		7	8½	
7	6		7	1	
8	7		7	6½	
9	8		7	4½	
10	9		6	6½	
11	10		6	7¾	
12	11		5	11	
13	12		* 5	8½	* The higheſt Point it would ſtand at for ſome Time.
	12		† 4	5½	† The loweſt Point; at which Points it would continue for ſome time.
14	13		4	4	
15	14		3	8	
16	14	1	* 4	2	* The higheſt Point.
	14	1	† 3	2	† The loweſt Point.
17	15		3	3½	
18	15	1	2	10	

He did not bleed half a Pint more after this laſt Trial, before he expired.

4. We may obſerve, that as this Horſe was more lively than the Mare, ſo the Blood mounted at firſt Trial ſeventeen Inches higher in the Tube, than the Mare's Blood did; yet there flowed three Pints of Blood leſs from

the Horſe than from the Mare: One Reaſon of this may be, that as ſhe was four Inches taller than the Horſe, ſo ſhe was probably proportionably bigger in Size every way, and ſhould therefore have more Blood; beſides, Bulk for Bulk, the Females are obſerved to have more Blood than the Males.

5. As the Quantity of Blood decreaſed, ſo would the projectile Force of the Blood in the Tube proportionably decreaſe; ſo that it would not riſe above a quarter of an Inch at a Pulſe, when the Horſe grew very faint.

6. The great Aſcents or Deſcents of the Blood, *viz.* twelve or fifteen Inches at a Time, did not ſeem to be owing immediately to the more vigorous or faint, faſter or ſlower Pulſation or Siſtole of the Heart; but by its continued equable beating, ſeemed rather to be occaſioned by a more or leſs Quantity of Blood flowing in to ſupply the left Ventricle of the Heart.

7 The Horſe's Pulſe beat forty Strokes in a Minute, before he was diſturbed or tied down: But when the Glaſs Tube was fix'd to the Artery, it beat ſixty five in a Minute. And as the Horſe grew fainter, the Pulſe was more and more accelerated, ſo as to beat an hundred times,

or

or more in a Minute: Whence we ſee, that the Pulſe is weak and quick, when the Heart is ſupplied with little Blood; which is the Caſe in the Hectick Fevers, *&c.*

8. And the Diaſtole of the Heart muſt neceſſarily be proportionably ſmall; for if the Heart dilated as much, when a ſmall Quantity of Blood flowed into the Ventricle, as when a large Quantity entered, it muſt then conſequently be filled partly with Air each Time, which would ſoon cauſe the Death of the Animal.

Experiment III.

1. IN *December* I laid a common Field Gate on the Ground, with ſome Straw upon it, on which a white Mare was caſt on her right Side, and in that Poſture bound faſt to the Gate; ſhe was fourteen Hands and three Inches high; lean, tho' not to a great Degree, and about ten or twelve Years old. This and the above-mentioned Horſe and Mare were to have been killed, as being unfit for Service.

2. Then laying open the left Jugular Vein, I fixed to that part of it which comes from the

the Head, a Glaſs Tube, which was four Feet, and two Inches long.

3. The Blood roſe in it, in three or four Seconds of Time, about a Foot, and then was ſtationary for two or three Seconds; then in three or four Seconds more, it roſe ſometimes gradually, and ſometimes with an unequally accelerated Motion nine Inches more, on ſmall Strainings of the Mare: Then upon greater Strainings it roſe about a Yard, and would ſubſide five or ſix Inches: Then upon a larger Strain or Struggle of the Mare, it roſe ſo high, as to flow a little out at the Top of the Tube; ſo that had the Tube been a few Inches higher, it would have riſen probably to that Height.

4. When the Mare ceaſed to ſtrain and ſtruggle, the Blood ſubſided about eighteen or twenty Inches; ſo the Return of the Blood into the Vein was not hindred by the Valves; which I have alſo obſerved in other Parts where there are Valves, tho' ſometimes they abſolutely hinder the Return of any Fluid.

5. The Diameter of the Braſs Pipe and Tube which were fixed to the Vein, were nearly one ſeventh of an Inch: The Diameter of the Jugular Vein about half an Inch.

6. Then

6. Then laying bare the left Carotid Artery, I fixed to it towards the Heart the Braſs Pipe, and to that the Wind-Pipe of a Gooſe; to the other End of which a Glaſs Tube was fixed, which was twelve Feet nine Inches long. The Deſign of uſing the Wind-Pipe was by its Pliancy to prevent the Inconveniencies that might happen when the Mare ſtruggled; if the Tube had been immediately fixed to the Artery, without the Intervention of this pliant Pipe.

7. There had been loſt before the Tube was fixed to the Artery, about ſeventy cubick Inches of Blood. The Blood roſe in the Tube in the ſame manner as in the Caſe of the two former Horſes, till it reached to nine Feet ſix Inches Height. I then took away the Tube from the Artery, and let out by Meaſure ſixty cubick Inches of Blood, and then immediately replaced the Tube to ſee how high the Blood would riſe in it after each Evacuation; this was repeated ſeveral times, till the Mare expired, as follows, *viz.*

The

	The ſeveral Trials.	Cubick Inches let out.	Perpendicular Height after each Evacuation. Feet.	Inches.
	1	70	9	6
	2	130	7	10
	3	190	7	6
	4	250	7	3
	5	310	6	5
	6	370	4	9
	7	430	3	9
	8	490	3	4½
	9	550	2	9½
	10	610	3	2½
* Deep Sighing raiſed the Blood.	11	670	*4	5
			2	9½
When the Force of the Blood was thus ſmall, then faint Sweats came on.	12	730	3	6
	13	790	3	5
Very faint.	14	820	2	0
Now expired.	15	833	2	5

8. We may obſerve, that theſe three Horſes all expired, when the perpendicular Height of the Blood in the Tube was about two Feet.

9. Theſe 833 cubick Inches of Blood weigh 28.89 Pounds, and are equal to fourteen Wine Quarts, the large Veins in the Body of the

the Mare were full of Blood, there was ſome alſo in the deſcending *Aorta*, and in both Ventricles and Auricles.

10. In order to make an Eſtimate, with what Force the Heart of this Mare muſt propel the Blood, to raiſe it in the Tube to the Height of nine Feet ſix Inches, I injected the right Ventricle of the Heart in the following manner, *viz.*

11. I fixed a musket Barrel to the pulmonary Vein, near its Entrance into the left Auricle; and tyed the aſcending and deſcending *Aorta's* faſt, at ſome Diſtance from their branching off from each other: Then placing the Barrel in a perpendicular Poſture, with a Funnel on the top of it, I poured in melted Bees-wax, till the Funnel was half filled. Yet as I had found by Experience, this perpendicular Height of melted Wax, which was near four Feet, would not have filled the Auricle and Ventricle, if I had not at the ſame time taken care to paſs a ſmall braſs Pipe, thro' one of the aſcending Branches of the *Aorta*, into the left Ventricle; thro' which the Air paſſed off as the Wax entered into the Ventricle, the braſs Pipe being at the ſame time gradually drawn up by

an Aſſiſtant, who as ſoon as all the Air was driven out tyed that Branch of the *Aorta* to prevent the flowing out of the Wax.

12. I choſe this Method of injecting from a perpendicular Height rather than by a Syringe, both becauſe I was by this means aſſured of the Force with which the injected Cavity of the Heart was dilated, which is more uncertain with a Syringe; and alſo becauſe this dilating Force from the perpendicular Height, continued acting uniformly till the Wax was grown ſtiff and hard.

13. When cutting open the left Ventricle, I found the Thickneſs of its muſcular Coat to be $1+\frac{1}{2}$ Inch, and the thinneſt Part of that of the right Ventricle was half an Inch.

14. Then taking out the Wax which was formed in the Shape of the Ventricle, I cut the Wax of the left Ventricle off, where the Valves called *Mitrales*, made the Separation; which Valves were propell'd inward by the entring Wax; and I did the ſame alſo at the Orifice of the *Aorta*, where the Valves called *Semilunares* were alſo propelled inward by the abovementioned braſs Vent-Pipe.

15. And this is the proper Cavity of the left Ventricle, juſt before its Contraction; for at that

that inſtant, the Blood flowing in from the Auricle has opened the mitral Valves inward, while at the ſame time the contracting Arteries repell the Blood forcibly againſt the ſemilunar Valves; but at the Inſtant that the Ventricle contracts, the mitral Valves are cloſed, being expelled by the Blood outwards, while at the ſame time the ſemilunar are by the ſame Action opened outwards, to make way for the compreſſed Blood to ruſh into the *Aorta.*

16. So that this Piece of Wax thus formed, may reaſonably be taken to be nearly commenſurate to the Quantity of Blood received into this Ventricle at each *Diaſtole*, and is thence propelled into the *Aorta* at the ſubſequent *Syſtoles.*

17. Having therefore filled a narrow mouthed Veſſel brim-full of Water, I immerſed the Wax in it, then taking it out of the Water, I filled the Veſſel brim-full again, from another Veſſel whoſe Capacity was divided into cubic Inches, which gave the Bulk of the Wax, and conſequently the Capacity of the left Ventricle, equal to ten cubic Inches.

18. I got the Quantity of the Surface of the Sides of this Ventricle by laying Pieces

of Paper aptly cut to the irregular Form of the ſeveral Parts of the Wax; and then laying thoſe Papers under another Paper which was equally divided into little Squares of $\frac{1}{4}$ Inch each; by running a Pin thro' both Papers at every Corner of each Square, the under Papers being thus marked too, it was eaſy by numbering their ſeveral Squares and Parts of a Square, to come pretty nearly to an Eſtimate of the whole inward Surface of the Ventricle: Which I by this means found to be equal to 26 ſquare Inches, deducting one ſquare Inch for the Area of the Orifice of the *Aorta*, whoſe Diameter I meaſured from the injected Wax.

19. The Diameter of the *Aorta* juſt before the coronary Artery branches from it, was 1.15 Inch.

Whence its Area 1.036 ſquare Inch.

The Diameter of the deſcending *Aorta* 0.93, its Area 0.677.

The Diameter of the aſcending *Aorta* 0.74, its Area 0.369.

20. The inward Area of the Sides of the left Ventricle being therefore equal to 26 Square Inches; the Sum of the whole Preſſure of the Blood againſt all the Sides of that Ventricle,

Ventricle, at the Inſtant when it begins firſt to contract, ſo as to ſuſtain the Preſſure of the arterial Blood, will be that Surface or Area multiplied into the perpendicular Height of the Blood in the glaſs Tube, *viz.* 26 × 114 Inches, *viz.* 2964 cubic Inches of Blood.

21. And a cubic Inch of Blood weighing 267.7 Grains, theſe multiplied into 2964 the Number of cubic Inches, and then reduced into Pounds gives 113.22 Pounds, which is the Sum of the Preſſure of the Blood, which this Ventricle ſuſtains, at the Inſtant when it is going to exert a contractive Force, ſufficient to propel it with conſiderable Velocity into the *Aorta*.

22. The Scruple Avoirdupoiſe contains 18.25 Grains, the Ounce 238 Grains, the Pound 7008 Grains.

23. The Area of the greateſt Section of this Ventricle from *Apex* to Baſe being 6.83 ſquare Inches; theſe multiplied into 114 Inches, the perpendicular Height of the Blood in the Tube, give 778.63 cubic Inches of Blood, equal to 29.7 Pounds; the Force of the Blood which the muſcular Fibres in that tranſverſe Section of the Ventricle muſt reſiſt.

24. The Velocity with which the Blood is

thrown out of the Ventricle into the Orifice of the *Aorta* may be thus computed, *viz.* the Capacity of this Ventricle being equal to ten cubic Inches, and the Area of the tranſverſe Section of the *Aorta* being 1.036, by which dividing the ten cubic Inches, the Quotient 9.64 is the Length of the Cylinder of Blood, which is formed in paſſing thro' the *Aorta*'s Orifice, at each *Syſtole* of the Ventricle. And a Horſe's Ventricle of his Heart contracting or his Pulſe beating 36 times in a Minute, that is 2160 times in an Hour, then a Column of Blood ſo many times 9.64 Inches, or 20819.4 Inches long or 1734.9 Feet will paſs in an Hour.

25. But the *Syſtoles* of the Ventricle during which that Quantity of Blood is propell'd, being eſtimated to be done in one third of the Space of time between each Pulſe, the Velocity of the Blood during each *Syſtole* will be thrice as much, *viz.* at the Rate of 5204.7 Feet, *i. e.* 0.98 of a Mile in an Hour or 86.7 Feet in a Minute.

26. Now this Velocity is only the Velocity of the Blood at its firſt entering into the *Aorta*, in the Time of the *Syſtole*; in conſequence of which the Blood in the Arteries,

being

being forcibly propelled forward, with an accelerated *Impetus*, thereby dilates the Canal of the Arteries, which begin again to contract at the Inſtant the *Syſtole* ceaſes: By which curious Artifice of Nature, the Blood is carried on in the finer Capillaries, with an almoſt even Tenor of Velocity, in the ſame manner as the ſpouting Water of ſome fire-Engines, is contrived to flow with a more even Velocity, notwithſtanding the alternate *Syſtoles* and *Diaſtoles* of the riſing and falling *Embolus* or Force; and this by the means of a large inverted Globe, wherein the compreſſed Air alternately dilating or contracting, in Conformity to the workings to and fro of the *Embolus*, and thereby impelling the Water more equably than the *Embolus* alone would do, puſhes it out in a more nearly equal Spout.

27 And ſince the Blood in the fineſt capillary Arteries, preſſes into the Veins with a much more equable Velocity, than in the *Aorta* and greater Arteries; ſince alſo the *Syſtole* is ſuppoſed to be nearly one third of the Time between Pulſe and Pulſe, the other two thirds of that Time muſt be ſpent in the Contraction of the Arteries: it may therefore reaſonably be concluded, that the Sum of the

 Dilata-

Dilatation of all the Arteries in each *Systole* is equal to about the Quantity of two thirds of the Blood, which is thrown out in each *Systole*, which in the Case of this Mare is equal to two thirds of ten cubic Inches, *viz.* 6.66.

28. This Ventricle throwing out ten cubic Inches at a time, will in the thirty six Pulses of a Minute throw out 360 cubic Inches, equal to 13.75 Pounds, and in an Hour 825 Pounds weight of Blood, nearly equal to the Weight of the Horse.

29. The Area of the transverse Section of the *Aorta* being as above noted 1.036 Inch: And the immediate next Divisions of it being in the Area of the like Section of the descending *Aorta* 0.677 Inch; and that of the ascending *Aorta* being 0.369, we find the Sum of the two Areas of these ascending and descending Branches is greater than that of the Trunck they arise from, and accordingly the Velocity of the Blood will be proportionably abated in them; as also on account of what passes through the coronary Arteries, before the Blood arrives at those two Branches, of which the descending *Aorta* is considerably the largest, thereby to furnish a greater Quantity of Blood

Blood, in the Proportion that all the Parts of the Body below the Heart, exceed the Bulk of thoſe above the Heart.

Experiment IV.

1. I Injected alſo with Wax the left Auricle and Ventricle of an Ox's Heart, which Ox was by gueſs ſuppoſed to weigh about 1600 Pounds when alive.

The Capacity of this Ventricle was equal to 12. 5. cubic Inches.

The Area of the tranſverſe Section of the *Aorta* equal to 1.539 Inch.

That of the deſcending *Aorta*, equal to 0.912, that of the aſcending equal to 0.85.

2. The Pulſe of a very gentle Cow, which was not terrified nor diſturbed, while its Pulſe was counted, was at the rate of thirty eight in a Minute nearly the ſame as that of a Horſe.

3. The Capacity of this Ventricle 12.5 being divided by the Area of the Orifice of the *Aorta* 1.539, the Quotient 8.1. Inches is the length of the Cylinder of Blood, which is formed in paſſing through the *Aorta* in each *Syſtole* of the Ventricle.

4. And an Ox's Pulse beating, or this Ventricle contracting 38 times in a Minute, that is 2280 times in an Hour, then a Column so many times 8.1 inches, or 18468 Inches long, or 1539 Feet will pass in an Hour.

5. But each *Systole* of the Ventricle being performed in one third of that time, the Velocity of the Blood in each *Systole* will be thrice as great, *viz.* 4617 Feet, *i. e.* 0.874 of a Mile in an Hour, or 76.95 Feet in a Minute.

6. This Ventricle throwing out 12.5 cubic Inches at a time, will in thirty eight *Systoles*, which it performs in a Minute, throw out 18.14 Pounds, and in an Hour and twenty eight Minutes, it will have thrown out 1600 Pounds of Blood, a Quantity equal to the Weight of the Ox. But this Ox being fat a Quantity of Blood equal to his Weight must be longer in passing through its Heart, than in the lean Horse, *Exper.* III. *Numb.* 27. for the Fat of Animals has little or no Blood in it, whence lean Animals have *cæteris paribus* much more Blood in them than fat ones.

EXPERI-

EXPERIMENT V.

1. I took an Eſtimate alſo of the Force of the Blood in a fat gelt Sheep or Wether, by fixing Glaſs Tubes to the jugular Vein and carotid Artery, in the ſame manner as I had done to the Horſe in *Exper.* III. The Sheep was three Years old, and weighed ninety one Pounds alive.

2. Its Pulſes beat ſixty five times in a Minute.

3. The Blood roſe in the Tube fixed to the jugular Vein $5+\frac{1}{2}$ Inches, and nine Inches when the Sheep ſtruggled and ſtrained.

4. In the Tube fixed to the carotid Artery it roſe ſix Feet $5+\frac{1}{2}$ Inches.

5. The Capacity of the left Ventricle of its Heart, was equal to 1.85 cubic Inch.

6. Its inward Surface =12. 35. ſquare Inches.

7. Its greateſt tranſverſe Section =2.54.

8. The Area of the tranſverſe Section of the Aorta =0.172. ſquare Inch, that of the deſcending *Aorta* =0.094, that of the left carotid Artery =0.012, and of the right =0.07, they both roſe ſeparate immediately from the *Aorta.*

9. The

9. The inward Surface of this left Ventricle being equal to 12 square Inches; this multiplied by six Feet 5+½ Inches produces 930 cubic Inches of Blood =35.62 Pounds, the Weight of Blood which this Ventricle sustains, just before its *Systole* begins.

10. And the Area of its greatest transverse Section being =2.54 square Inches, this multiplied into six Feet 5+½ Inches the height of the Blood in the Tube, the Product is 393.7 cubic Inches of Blood =15.03 Pounds, the weight of Blood which the Fibres in this transverse Section of the Ventricle must sustain.

11. The Capacity of the left Ventricle being =1.85 cubic Inch, which divided by 0.172, the Area of the transverse Section of the *Aorta*, the Quotient 10.75 is the length of the Cylinder of Blood, which is formed in passing through the *Aorta* in each *Systole* of the Ventricle.

12. And this Sheep's Pulse beating, or his left Ventricle contracting sixty five times in a Minute, that is 3900 times in an Hour, therefore a Column of Blood so many times 10.75 Inches, or 41875 Inches long, or 3489.5 Feet will pass in an Hour.

13 But

13. But the *Syſtoles* of the Heart, during which that Quantity of Blood is propelled, being eſtimated to be done in one third of the Space of Time between each Pulſe, the Velocity of the Blood during each *Syſtole* will be thrice as much, *viz.* at the Rate of 10468.5 Feet, *i. e.* 1.98 Mile in an Hour, or 174.4 Feet in a Minute.

14. And the Ventricle throwing out 1.85 cubic Inch of Blood each Time, that will be 4.593 Pounds in a Minute, or ninety three Pounds, a Quantity equal to the Weight of the Sheep in twenty Minutes.

EXPERIMENT VI.

1. HAving fixed a Tube to the left crural Artery of a fallow Doe, the Blood roſe four Feet two Inches in the Tube.

2. I injected with Wax both Auricles and Ventricles of the Heart of another Doe, and found the Capacity of the Ventricle equal to nine cubic Inches, and the right Auricle and Ventricle near as big.

3. Timorous Animals are obſerved to have larger Hearts than couragious ones; as Deer,

Aſſes,

Aſſes, Hares, *&c.* which holds true in the Inſtance of this Doe's Heart. *Qu.* May not one Reaſon of this be, that the Fibres of the timorous are generally more lax than thoſe of couragious Animals, on which Account the Blood paſſing with leſs Reſiſtance through the lax fibred capillary Veſſels, it was requiſite that the Heart ſhould at each Pulſe throw out a greater Quantity of Blood, in order to ſupply its more eaſy and plentiful Flow through the lax capillary Arteries into the Veins? And may not this be the Reaſon why the Pulſes of young Animals, as of Children, are found to beat faſter than thoſe of grown Perſons, *viz.* becauſe the tender Fibres of the Coats of their Blood Veſſels being very lax, they give the leſs Reſiſtance to the flowing Blood; whoſe Globules are obſerved by *Leewenhoeck* to be all of a Size both in great and ſmall Animals; whence it was needful to make Proviſion for a proportionably greater Supply of it from the Heart, by increaſing the Velocity of the Dilatations and Contractions of that curious Engine; in the Formation of which are ſeen ſuch evident Marks of the conſummate Wiſdom of the great Author of Nature.

4. The Area of the tranſverſe Section of the *Aorta* of this Doe $=0.476$ of the deſcending *Aorta* $=0.383$, of the aſcending $=0.246$, and that of the pulmonary Artery $=0.502$. But it being not eaſy to obtain, in that timorous Creature, the juſt Number of Pulſes in a Minute; I could not calculate the Velocity of the Blood, nor the Quantity that paſſes in any determinate time.

EXPERIMENT VII.

1. I Fixed Tubes alſo in the ſame manner to the jugular Vein and carotid Artery of ſeveral Dogs; for whatever Experiment I principally intended to make on any Dog, I uſually began with fixing a Tube firſt to the jugular Vein, and then to the carotid Artery, which was the Method I uſed to waſh the Blood out of the capillary Veſſels, thereby the better to prepare them for my intended Experiments.

2. The Force of the Blood in the Veins and Arteries is very different, not only in Animals of different Species, but alſo in Animals of the ſame kind, and that not only in thoſe of different Sizes and Weights, but alſo in Dogs of the ſame Size and Weight; and

even

even in the same Animal the Force of the Blood in its Vessels, is continually varying, according to the different Kinds and Quantities of Food, the various Distances of time after taking Food, the more or less plethoric State of the Blood Vessels, also from Exercise, Rest, different States of Vigour or Vivacity of the Animal, and many other Circumstances, which may conduce to vary the Force of the Blood: For the healthy State of Animals is not confined to the scanty Limits, of one determinate Degree of vital Vigour in the Blood: But the Allwise Framer of these admirable Machines has so ordered it, as that their healthy State shall not be disturbed by every little Variation of this Force, but has made it consistent with a very considerable Latitude in the Variation of it. Now since this Force of the Blood is so variable, it is the more requisite to be furnished with a good Quantity of Observations, thereby to find out the more nearly, a Medium of those Forces, not only in the same Animal, but also in those of different Ages, Sizes and Kinds; whence haply some curious Observations may arise.

3. These great Inequalities of the Force of the Blood, not only in different Animals, but

alſo in Animals of the ſame, kind may be ſeen in the following Table in *Exper.* VIII. *Numb.* 12. in which I have ſet down the Weights of moſt of them, and alſo in different Columns, the Height to which the Blood roſe in Tubes fixed to the Veins and Arteries.

4. I obſerved here, as in the abovemention-ed Horſes, that when the Blood had ſubſided a little in the Tubes which were fixed to the Arteries of theſe Dogs, it would, as in the Horſes, riſe on a ſudden conſiderably on deep ſighing; as alſo on preſſing the Dogs Bellies hard with the Hand, the Blood would immediately riſe about ſix Inches, and ſubſide as much, on taking off the Hand, and it was the ſame on ſeveral Repetitions.

5. It may be objected to this Method of eſtimating the Force of the Blood, that by thus fixing Tubes to theſe large Veins and Arteries, the Courſe of a conſiderable Stream of Blood was for that time ſtopped; and that conſequently the Force of the Blood muſt be proportionably increaſed in all the Veins or Arteries; and therefore alſo in the Veins or Arteries to which the Tube is fixed. And doubtleſs in ſome degree it is ſo. In the Sheep the left Carotide is nearly $\frac{8}{13}$ part of the right Caro-

tide and deſcending *Aorta* taken together; and in the Dog, Numb. 3. it is about $\frac{1}{16}$ of them.

6. To obviate therefore this Inconvenience, I fixed Tubes laterally to the jugular Veins and Arteries of the Dog, Numb. 13. in the following manner, *viz.* I took two cylindrical Sticks which were $\frac{1}{2}$ Inch Diameter, and $1+\frac{1}{2}$ Inch in length; and having bored Holes through them from end to end, ſomething larger than thoſe Veins and Arteries; I then ſlit them in halves Length-ways, and bored another Hole thro' the middle of one of them into its Cavity, into which lateral Hole the braſs Pipe entered; which was at its other End adapted to fit another Pipe which was cemented to a glaſs Tube. Then having laid the Vein or Artery bare, I drew a linnen Cloth under it to wipe it very dry; and then placed under it one of the above-mentioned ſlit Pieces of Wood, laying the Vein or Artery in its Cavity, which was covered with Pitch, that was at that inſtant afreſh melted with a ſmall warm iron Rod; then pouring melted Pitch not very hot, over the Vein or Artery, I immediately put on the other half of the ſplit Wood, which had the Hole

bored thro' it, and tied them faſt together: Then entering the very ſlender Point of a Pen-knife into the abovementioned Hole, I cut an Orifice in the Vein or Artery, and then immediately fixed the braſs Pipe and Tube to receive the following Blood which roſe from the jugular Vein of the thirteenth Dog firſt ſix Inches, and on ſtraining $9+\frac{1}{2}$ Inches, and from the Artery four Feet eleven Inches, and would doubtleſs have mounted higher, if the Blood had not made an Outlet between the Artery and the Pitch, ſo as to prevent its Riſe; which Inconvenience might eaſily be prevented by proper Care; which if done would give us the real Force of the Blood againſt the Sides of the Arteries, as it did in this jugular Vein.

7. I believe this would be a good Method to take the Force of the Blood in leſſer Animals, where by reaſon of the Smallneſs of thoſe Veſſels it might be difficult to inſert Pipes into them, which if done thoſe Pipes would have too ſmall a Bore for the Blood freely to paſs thro' them.

8. I have noted in the following Table, *Exper.* VIII. *Numb.* 12. the ſeveral Heights to which the Blood roſe in Tubes fixed to the

the Veins and Arteries of Animals, as they lay horizontally on their Backs, or on one side in the Case of the Mare, *Exper*. III. But when an Animal stands on its Legs, a Column equal to the perpendicular Height of the Animal, must be added to the several Heights of the Blood in the glass Tubes, in order to estimate the Force, with which the Blood presses against the Coats of the Blood-Vessels, at the lower Parts of the Body, and so in proportion for any other Part that is higher. So that these Columns of Blood in the Arteries and Veins, communicating with each other below, are on account of their equal Heights, equipollent to each other : The progressive Motion of them being determined by the Energy of the Heart. And tho' Valves in Tubes in which a Fluid is propelled upwards with an equable Force, would rather retard than promote its Progress; yet in Tubes where the Fluid does not ascend equably, but by Reason of frequent Motions of the whole Machine, it is subject to many Agitations; in this case Valves are of great Importance to check the Repercussion and Regurgitation of the Fluid: And accordingly the allwise Framer of animal Bodies has provided Valves in the

Veins, to prevent this Inconvenience, and that principally in the lower Parts of the Body, where they are most needed, especially in great Motions, and in exerting the muscular Force of the Body.

EXPERIMENT VIII.

1. THE Blood having risen six Feet eight Inches from the crural Artery of the Dog, *Numb.* 1. and to the same Height from the left carotide Artery of *Numb.* 7. in the Table, *Numb.* 12. of this *Experiment* VIII. I chose to calculate the Velocity, *&c.* of the Blood of this Dog.

2. The Capacity of the left Ventricle of the Heart being injected with Wax was found equal to 1.172 cubic Inch.

3. Its inward Surface equal to eleven square Inches, which multiplied into the perpendicular Height of the Blood in the glass Tube, which was fixed to the Artery, *viz.* six Feet eight Inches or eighty Inches, gives eight hundred and eighty cubic Inches of Blood, which press on all Sides of that Ventricle, when it has contracted just so far, as to sustain and be equal to the Force of the Blood in the *Aorta.*

 4. These

4. Theſe eight hundred and eighty cubic Inches multiplied by 267.7 the Number of Grains in a cubic Inch of Blood gives 235576 = 33.61 Pounds.

5. The Area of the tranſverſe Section of the *Aorta*, juſt before the coronary Arteries branch off from it, being 0.196 ſquare Inch, by which dividing 1.172 cubic Inch, the Capacity of the Ventricle, the Quotient 5.978 Inches is the Length of the Cylinder of Blood, which is formed in paſſing thro' the Orifice of the *Aorta*, at each *Syſtole* of the Ventricle.

6. And a Dog's Pulſe being found to beat, or his left Ventricle to contract ninety ſeven times in a Minute, then a Column of Blood ſo many times 5.97 Inches long, will be 34745.4 Inches or 2895.45 Feet long: But the *Syſtoles* of the Heart during which that Quantity is propelled, being eſtimated to be done in one third of the Time between Pulſe and Pulſe, the Velocity of the Blood during each *Syſtole* will be thrice as much, *viz.* 8586.35 Feet, that is at the rate of 1.62 Mile in an Hour, or 143.1 Feet in a Minute.

7. And the Ventricle throwing out 1.172 cubic Inch of Blood in each *Syſtole*, that is 4.34 Pounds in ninety ſeven Pulſes, the Num-

ber

ber of Pulſes in one Minute, hence fifty two Pounds, a Quantity equal to the Dog's Weight, will paſs thro' the Heart in 11.9 Minutes.

8. If according to Dr. *Keill's* Eſtimate the left Ventricle of a Man's Heart throw out in each *Syſtole* an Ounce or 1.659 cubic Inch of Blood, and the Area of the Orifice of the *Aorta* be =0.4187; then dividing the former by this, the Quotient 3.96 is the Length of the Cylinder of Blood, which is formed in paſſing thro' the *Aorta* in each *Syſtole* of the Ventricle; and in the ſeventy five Pulſes of a Minute a Cylinder of 297 Inches Length will paſs this at the Rate of 1493 Feet in an Hour. But the *Syſtole* of the Heart being performed in one third of this Time, the Velocity of the Blood in that inſtant will be thrice as much; *viz.* at the rate of 4479 Feet in an Hour, or 74.6 Feet in a Minute.

9. And if the Ventricle throws out 1.172 cubic Inch in a Pulſe, then in the ſeventy five Pulſes of a Minute the Quantity of Blood will be equal to 4.37 Pounds, and in 36.3 Minutes a Quantity equal to a middle-ſized Man, *viz.* a hundred and ſixty Pounds will paſs thro' the Heart.

10. But if with Dr. *Harvey* and Dr. *Lower* we ſuppoſe two Ounces of Blood, that is 3.318 cubic Inches to be thrown out at each *Syſtole* of the Ventricle, then the Velocity of the Blood in entering the Orifice of the *Aorta* will be double the former, *viz.* at the rate of 149.2 Feet in a Minute, and a Quantity of Blood equal to the Weight of a Man's Body will paſs in half the Time, *viz.* 18.15 Minutes.

11. If we ſuppoſe, what is probable, that the Blood would riſe $7+\frac{1}{2}$ Feet high in a Tube fixed to the carotide Artery of a Man, and that the inward Area of the left Ventricle of his Heart, is equal to fifteen ſquare Inches, theſe multiplied into $7+\frac{1}{2}$ Feet give 1350 cubic Inches of Blood which preſſes on that Ventricle, when firſt it begins to contract, a Weight equal to 51.5 Pounds.

12. That we may the more readily compare the abovementioned ſeveral Eſtimates together, I ſhall here range them in order in a Table.

The Several Animals.	Weight of each.	Height of the Blood in the Tube from Jugul.	Height of the Blood in Tubes fixed to Arteries.	Capacity of the left Ventricle of the Heart.	Area of the Orifice of the Aorta.	Velocity of the Blood in the Aorta.
	Pd. Ou.	Inches.	Feet Inch	Cub. Inches.	Square Inches.	Feet Inch in a Minute.
Man	160	On ſtraining.	7 6	1.659 3.318	0.4187	74.6 149.2
Horſe 1ſt			8 3			
2d			9 8			
3d	825	12 52	9 6	10.	1.036	86.7
Ox	1600			12.5	1.539	76.95
Sheep	91	5½ 9	6 5½	1.85	0.172	174.4
Doe			4 2	9	0.476	
Dogs 1ſt	52	0 6	6 8	1.172	0.196	143.1
2d	24	5 7	2 8	1	0.185	130.9
3d	18	5	4 8	0.633	0.118	127.4
4	12 8	4	3 3	0.5	0.101	120
5		4 6	at crural Arter.	1.25	0.210	143
6	31				0.196	
7	43		6 8	1.172	0.179	156.5
8			6 6	Tube fixed to the crural Artery.		
9		7 14	3 1			
10	15	5 24	1 6	was very old and died ſoon.		
11	37	8½	4 9			
12	36		6 7			
13	24	6 9½	4 11	Tube fixed laterally to the left carotide Artery.		
14	37 8		5 8			
15		5 19	on ſucking at the Tube.			
16	———	5½ 8	on ſucking			
17	19	5 14	5 2			
18	35	5	4 7			
19	32	6 9½	3 11			
20	23	5 7	4 10			

The ſeveral Animals.	Quantities of Blood = to the Weight of the Animal in what Time.	How much in a Minute.	Weight of the Blood ſuſtain'd by the left Ventricle contracting.	Number of Pulſes in a Minute.	Area of the tranſverſe Section of deſcending Aorta.	Area of the tranſ. Section of aſcending Aorta.
	Minutes	Pounds	Pounds		Square Inches	Square Inches
Man	36.3 18.15	4.37 8.74	51.5	75		
Horſe 3d	60	13.75	113.22	36	0.677	0.369
Ox	88	18.14		38	0.912	0.85
						Ri. left
Sheep	20	4.593	35.52	65	0.094	0.07 0.012
					0.383	0.246
						Ri. left
Dog 1	11.9	434	33.61	97	0.106	0.041 0.034
2	6.48	3.7			0.102	0.031 0.009
3	7.8	2.3	19.8		0.07	0.022 0.009
4	6.2	1.85	11.1		0.061	0.015 0.007
					0.119	0.7 0.031
					0.125	0.062 0.031
7	6.56	4.19			0.109	0.053 0.032

13. I do not ſee, by comparing the Weights of theſe Animals, and the ſeveral Quantities of Blood which paſs thro' their Hearts in a given Time, that we can thence form any Rule that is fixed, for the proportioning the Quantities of flowing Blood to their different Sizes.

14. Theſe Quantities in larger Animals are very diſproportionate to the Bulk of their Bodies, in compariſon of what they are in leſſer Animals, as eſtimated in this Table.

15. But as in the bigger Animals the Blood has a longer Courſe to go, and muſt therefore meet with a greater Reſiſtance; ſo we may obſerve in this Table, by comparing the perpendicular Heights of the Blood in the Tubes fixed to the Arteries, that the Force of it in the Arteries, is in the main greateſt in the largeſt Animals.

16. And ſuppoſing the Blood-veſſels in the Man and Horſe to be equally diſtributed in all their homologous Parts, that is if they are proportionable to their reſpective Weights, then the Blood muſt move in them reciprocally as the Times, in which Quantities of Blood equal to their reſpective Weights, paſs thro' their Hearts, *viz.* as 60 to 15.15 Minutes.

17. So

17. So that notwithſtanding the arterial Blood of a Horſe is propelled with a greater Force than that of a Man; yet it moves the ſlower in the Horſe on account of a greater Number of Ramifications, and a greater Length of Veſſels in the larger Animal.

18. When I compared the Proportion, that the Area of the tranſverſe Sections of the deſcending *Aorta* bear to the Fleſh and other Parts which they ſupply with circulating Blood, I found it to be as follows, *viz.* having cut the Body of a Dog aſunder at his Heart, and firſt weighing each part ſeparately, and then boiling them ſo as to ſeparate the Bones from the Fleſh; the Weight of the Bones being deducted from the Fleſh, the Fleſh, *&c.* of the hinder part was found to weigh eleven Pounds eleven Ounces; that of the forepart ſeven Pounds two Ounces.

19. Now the Areas of the tranſverſe Section of the Arteries of theſe five Animals are by Meaſurement as follow, *viz.*

20. In

	Aorta.	deſc.	aſcend.		
20. In the Mare	—1.036	—0.677	—0.369	by computation	0.412
Ox	—1.539	—0.912	— 0.85	from the above	0.056
Sheep	—0.172	—0.094	—0.082	found Propor-	0.057
Doe	—0.476	—0.383	—0.246	tion of Fleſh in	0.234
1ſt Dog	—0.196	—0.106	—0.075	the hinder and	0.065
6th Dog	—0.196	—0.125	—0.093	fore parts.	
7th Dog	—0.179	—0.109	—0.085		

21. In this Table we find that the Areas of the tranſverſe Section of the deſcending and aſcending *Aorta's* of the firſt Dog are nearly proportionable to the Weights of the reſpective Parts which they ſupply with Blood; and that in the Mare and Doe the Difference is not great, but greater in the Ox and Sheep. In Eſtimates of this kind great Accuracy in the Proportions is not to be expected.

22. The Velocity with which the Blood is thrown out of the left Ventricle, being performed in one third of the time between *Syſtole* and *Syſtole*, the like Quantity of Blood would move with an equable Motion, of one third of that Velocity, thro' the Orifice of the *Aorta* in the Space of Time between each *Syſtole*.

23. Since in a Man a Cylinder of Blood of the Diameter of the Orifice of the *Aorta* and 7.92 Inches long, is at each Pulſe impelled thro' a dilatable conical Artery, its Velocity

Velocity would be greatly increaſed, by paſſing thro' that narrower Defilee; but the Arteries continually ſending off innumerable Branches, the Sum of whoſe Orifices are conſiderably larger than the main Stems, hence the Velocity of the Blood muſt be proportionably rebated. So that as Dr. *James Keill* in his *Tentamina Medico-Phyſica, p.* 46. has eſtimated it, the Velocity of the Blood at the Heart, would be to its Velocity in an evaneſcent Artery as 5233 to 1, if it had a free unimbarraſſed Courſe thro' thoſe capillary Arteries. And ſince the Velocity at its paſſing from the Heart to the *Aorta* is at the rate of 149.2 Feet in a Minute, taking one third of that, *viz.* 49.73 for its continued equable Velocity, according to Dr. *Keill's* Eſtimate it would move but 0.0095th part of a Foot or 0.083 Inch in a Minute, in the evaneſcent Arteries in that time.

24. This would be its Velocity there, if the Blood had as free and unimbarraſſed a Courſe thro' the finer capillary Arteries, as it has thro' their larger Ramifications. But by the following Experiment it is found, that the principal Obſtruction to the Progreſs of the arterial Blood is in the capillary Arteries.

EXPERI-

Experiment IX.

1. I Slit open with a Pair of Sciſſors, from end to end, the Guts of a Dog, on that ſide which was oppoſite to the Inſertion of the meſenterick Arteries and Veins; and having fixed a Tube $4+\frac{1}{2}$ Feet high to the deſcending *Aorta* a little below the Heart, I poured blood warm Water thro' a Funnel into the Tube, which deſcended thence into the *Aorta*, with a Force equal to that, with which the Blood is there impelled by the Heart: This Water paſſed off thro' the Orifices of innumerable ſmall capillary Veſſels, which were cut aſunder thro' the whole Length of the ſlit Gut. But notwithſtanding it was impelled with a Force equal to that of the arterial Blood in a live Dog, yet it did not ſpout out in little diſtinct Streams, but only ſeemed to ouze out at the very fine Orifices of the Arteries, in the ſame manner as the Blood does from the capillary Arteries of a Muſcle cut tranſverſely.

2. Having provided a Pendulum which beat Seconds, and pouring in thro' the Tube known Quantities of warm Water, I found

that

that 342 cubic Inches of Water paſſed off in 400 Seconds or 6.6 Minutes.

3. Then cutting all the meſenteric Arteries aſunder cloſe to the Guts, and taking away the Guts, I found that a like Quantity of Water paſſed thro' theſe larger Ramifications of the Arteries in 140 Seconds, or 2.3 Minutes, that is in one third of the Time.

4. Then cutting aſunder the crural Arteries, which were before tyed, and cutting off the meſenteric and emulgent Arteries cloſe to the *Aorta*, a like Quantity of Water paſſed thro' this thus cut *Aorta* in 0.308 Minute, that is in $\frac{1}{21.4}$th part of the Time, in which it paſſed thro' the capillary Arteries of the ſlit Guts.

5. There being 342 cubic Inches which paſſed thro' the capillary Arteries of the ſlit Guts in 6.6 Minutes, that is thirteen Pounds if it were Blood, or 1.939 Pound in a Minute: And it being eſtimated in the Table, *p.* 43. that 4.34 Pounds of Blood were thrown out of the Heart of the Dog, *Numb.* 1. in a Minute, the abovementioned 1.939 Pound is $\frac{1}{0.44}$th Part of what paſſes the Heart in that Time.

6. But on weighing all the fleshy and other membranous Parts of another Dog, thro' which the arterial Blood passes, that is, exclusive of the Bones and Lungs, I found the whole Weight to be eighteen Pounds eleven Ounces, of which the slit Gut weighing one Pound two Ounces was therefore $\frac{1}{16.6}$th part of the whole: So that Bulk for Bulk there passed 30.27 times more Water thro' the Arteries of these slit Guts than thro' the rest of the Arteries of the Body, and that with a Force no greater than that of the Heart.

7. Which may reasonably be attributed to these several Causes, as to the much greater Fluidity of Water than of viscid Blood, to the more relaxed State of these Arteries in the dead than in the living Animal; for tho' the Arteries and Veins of a dead Animal being then freed from the distending Force of the Blood do contract, yet with equal Forces, those of the dead Animal will dilate more than those of a live Animal: But this more plentiful Flow of Water is principally owing to the great Difference there is in Size, between these cut capillary Arteries, and the succeeding Series's of exceeding small Ramifications, and that at right Angles, thro' which the Blood passes

in its further Progreſs towards the Veins: As alſo to a want of the Reſiſtance of the venal Blood, which riſing ſix Inches in the Tube fixed to the jugular Vein is $=\frac{1}{13.33}$d part of the Force of the arterial Blood, and muſt therefore proportionably retard its Motion.

8. The Diameters of the cut Orifices of the Arteries thro' which the Water paſſed off, were at a Medium, one with another, equal to twice the Diameter of a Hair, which Dr. *Jurin* by an accurate Eſtimation found to be $\frac{1}{324}$th part of an Inch; hence theſe Arteries which are $\frac{1}{162}$ Inch Diameter, as they branch off from the Meſenterics ſpread themſelves alternately, on each ſide of the Guts, whence meeting again, their inoſculating Branches form *Areola's* like thoſe that are on the Leaves of Trees: And from theſe thus converging Arteries and ſap Veſſels, there branch off nearly at right Angles, without converging any more, much ſmaller Arteries, and from theſe others again, both at right Angles, and like the ſpread Fingers of a Hand, in ſucceſſive Series's in their Progreſs towards the Veins.

9. The Diameters of the firſt Series's of theſe unconverging Branches, may in a Piece of Gut well injected with Vermilion, be ob-

ſerved to be of ſeveral Sizes from $\frac{1}{2}$ to $\frac{1}{3}$ of the Arteries whence they roſe, and the ſucceeding ones finer and finer to nearly $\frac{1}{3240}$th part of an Inch, that is ſo fine, that only ſingle Blood globules can paſs them into the Veins; here therefore ſo viſcid a Fluid as the Blood muſt needs meet with a very great Reſiſtance.

10. If we compare the Area of the Orifice of the braſs Pipe, which was in this Experiment fixed to the deſcending *Aorta*, with the Sum of the Areas of all the large meſenteric Arteries juſt before they ſubdivide into Branches, which immediately enter the Coats of the Guts, and alſo with ſome of the Areas of the cut Orifices of the above-mentioned converging Arteries, they are as follow, *viz.*

11. The Area of the Orifice of the braſs Pipe was 0.057 ſquare Inch, taking a mean Diameter of one of the meſenteric Arteries to be 0.06 Inch, its Area 0.0028, and counting 82.8 of theſe in $11 + \frac{1}{2}$ Feet Length of the ſlit Gut, the Sum of all their Areas will be 0.234 Inch.

12. Taking alſo the Diameter of one of theſe Arteries at their firſt entering the Guts to be equal to 0.02, its Area will be 0.000314, and the Sum of 724.25 of theſe Areas in $11 + \frac{1}{2}$ Feet length of Gut will be 0.227.

14. And

13. And a mean Diameter of a converging Artery at the slit part of the Gut being $\frac{1}{162}$ Inch equal to 0.00672, its Area will be 0.000135, and the Number of all these Arteries in 11 $+\frac{1}{2}$ Feet length of Gut being 1695, the Sum of the Areas of all their Orifices at the slit part will be 0.2288 square Inch.

14. If we take a small converging Artery whose Diameter is $\frac{1}{324}$ equal to 0.000308, its Area will be 0.00000744; I have observed from each side of such an Artery, four Branches to go off at right Angles, each of whose Diameter was half of the converging Artery, *viz.* $\frac{1}{648}$ Inch equal to 0.00154 whose Area is 0.00000218, which multiplied by 8, the Number of those Branches, the Sum of their Areas is 0.00001744, which exceeds the former by 0.00001.

15. These reticular converging Arteries, by being thus inosculated into each other, both prevent Obstructions in them, and also thereby the more plentifully supply the next Series of rectangular Branches with Blood; for if the Blood had entered the converging Arteries only at one end, its Velocity would thereby have been more retarded in going the whole Length than half the Length of these converg-

ing Arteries; by theſe innumerable Convergencies of the Arteries the Blood is more blended and mixed, as is plain to be ſeen in the Lungs of Frogs.

16. By comparing the Sum of the tranſverſe Section of thoſe ſeveral meſenteric and converging Arteries together, we may obſerve that the firſt and ſecond Order of the meſenteric Arteries are nearly equal, *viz.* 0.228 0.234. 0227, yet equal Quantities of Water were found to paſs in one third of the time thro' the cut meſenteric Arteries, that they did thro' the cut converging Arteries of the ſlit Guts, that is ſince the Quantities which flow thro' equal Tubes are reciprocally as the times, the Quantities in theſe converging Arteries ought to have been as 981.38 to 342 cubic Inches, whereas there flowed but $\frac{1}{2.86}$th part.

17. And tho' the Area of the Orifice of the braſs Pipe, which was fixed to the *Aorta* was but 0.057 that is but $\frac{1}{4}$ part of the Sum of the other Arteries, yet 1148.9 cubic Inches would have paſſed that in 6.6 minutes, when the great Branches of the *Aorta* were cut off, that is, 1.17 times as much would in that time flow thro' it as thro' the meſenteric Arteries, and 3.3 times as much, as did paſs thro'

thro' the cut Orifices of the converging Arteries.

18. Hence we ſee how greatly the Velocity of the Water is retarded in paſſing thro' the ſeveral Branchings of the Arteries, notwithſtanding the Sum of the Areas of their tranſverſe Section is conſiderably greater than that of the *Aorta*. And this Retardation muſt be ſtill greater to the Blood which is both a groſſer and more viſcid Fluid than Water, and that eſpecially in the extream capillary Arteries which branch off at right Angles, and which are about $\frac{1}{1620}$th part of an Inch in Diameter, ſo fine that only ſingle Globules of Blood can paſs them.

19. And to this Reſiſtance which the Blood meets with in paſſing the capillary Arteries, is owing the great Difference of the Force of the Blood in the Arteries to that in the Veins, *viz.* as 10 or 12 to 1.

20. For tho' the Velocity of the Blood at its firſt Entrance into the *Aorta*, depends on the Proportion the Area of its Orifice bears to the Quantity thrown into it at each *Syſtole*, and alſo on the Number of thoſe *Syſtoles* in a given time: Yet the real Force of the Blood in the Arteries, depends on the Proportion, which

the Quantity of Blood thrown out of the left Ventricle in a given time, bears to the Quantity which can paſs thro' the capillary Arteries into the Veins, in that time.

21. But the Reſiſtance which the Blood meets with in thoſe capillary Paſſages, may be greatly varied, either by the different Degrees of the Viſcidity or Fluidity of the Blood, or by the ſeveral Degrees of Conſtriction or Relaxation of thoſe fine Veſſels; Inſtances of which may be ſeen in Experiments XV, XVI, XVII, XVIII.

22. And as the State of the Blood or Blood-veſſels are in theſe Reſpects continually varying from divers Cauſes, as Motion, Reſt, Food, Evacuations, Heat, Cold, *&c.* ſo as probably never to be exactly the ſame, any two Minutes, during the whole Life of an Animal; ſo nature has wiſely provided, that a conſiderable Variation in theſe, ſhall not greatly diſturb the healthy State of the Animal.

23. We may make a pretty near Eſtimate of the Force of the Blood in the capillary Veſſels in the following manner, *viz.* taking the Diameter of a Blood Globule to be as above $\frac{1}{3240}$th part of an Inch; which *Leewenhoek* has obſerved to be of the ſame Size both in

in ſmall and great Animals; and allowing theſe capillary Veſſels to be a ſmall Size larger than the Globules, which ſwim in and are carried along by *Serum* which ſurrounds them on all ſides, we may therefore well ſuppoſe one of theſe Veſſels to be double the Diameter of ſuch a Globule, *viz.* $\frac{1}{1620}$th part of an Inch or 0.00617, the Periphery therefore of this Veſſel will be 0.0194, and its Area 0.0000298, which multiplied by 80, the Number of Inches to which the Blood roſe in the Tube when fixed to the Artery of the Dog Numb. 1, gives 0.0000298 parts of eighty cubic Inches of Blood or of 21416 Grains, equal to 1.997 Grains. But the Reſiſtance of the Blood in the Veins of the ſame Dog being found equal to ſix Inches Height or $\frac{1}{12.5}$th part of eighty Inches, this $\frac{1}{1..5}$th part being deducted out of 1.997 Grain, the Remainder 1.838 Grain is the Force with which the Blood would be impelled into ſuch a Capillary by a Column of Blood of eighty Inches Height, ſuppoſing it were in a ſtagnant State; to which alſo muſt be added the Velocity which the Blood has acquired at its firſt Entrance in the capillary Veſſel, which can be but ſmall as appears by the great Reſiſtance it meets with in the capil-

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lary Veſſels, in this IXth Experiment, Numb. 18. whence we ſee both from Experiment and Calculation, that the Force of the Blood in theſe fine Capillaries can be but very little, and the longer ſuch Capillaries are, the ſlower will the Motion of the Blood be in them.

24. It is obſervable that theſe parallel Arteries are not as in the Bowels, Lungs and other membranous parts of the Body, intermixed with correſponding ſimilar Veins ; but two different Series's of theſe Arteries, ariſing at right Angles from larger Arteries, one Series from the upper, and the other from the lower parts of the Muſcles, their parallel Arteries do mutually and alternately intermix, whereby the Blood is conveyed in them alternately upwards and downwards, and thence flows at right Angles into the Veins.

25. From this very ſmall Force of the arterial Blood among the muſcular Fibres we may with good reaſon conclude, how ſhort this Force is of producing ſo great an Effect, as that of muſcular Motion, which wonderful and hitherto inexplicable Myſtery of Nature, muſt therefore be owing to ſome more vigorous and active Energy, whoſe Force is regulated by the Nerves : But whether it be con-

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fined in Canals within the Nerves, or acts along their Surfaces like electrical Powers, is not easy to determine.

26. That a vibrating electrical Virtue can be conveyed and freely act with considerable Energy along the Surface of animal Fibres, and therefore on the Nerves, is evident from curious Experiments, made by that skilful and indefatigable Experimenter Mr. *Stephen Gray*, of which he has given an Account in the *Philosophical Transact. Numb.* 417. 422, where he shows that electrical Virtues from a Glass heated by rubbing, will not only be conveyed along the Surface of Lines to very great Lengths, but will also be freely conveyed from the Foot to the extended Hand of a human Body suspended by Ropes in a horizontal Posture in the Air; and also from that Hand to a long Fishing Rod held in it, and thence to a String and a Ball suspended by it: And also that an electrical Virtue may be carried along a Surface of Water.

27. And it has been frequently observed, that when some part of the Body has upon itching been gently scratched by the Nails, there has at the same time been felt in a distant part, a very pungent *Stimulus* or Sensation answering

exactly

exactly Stroke for Stroke to the Action of the ſcratching Nails. Thus particularly on ſcratching a ſmall Pimple, a little below the right Knee on the outſide, a like pungent Senſation has been felt on the left Shoulder Blade, and ſometimes on that Arm ſome Inches below the Shoulder: And *vice verſa* the right Shoulder or Arm has in like manner been affected, when the ſcratching has been made near the left Knee, but this Effect does not always follow: There are many Inſtances of the Sympathy of the Nerves.

28. That the animal Spirits, whether they act within or on the Outſides of the Nerves, are elaſtick, ſeems probable not only from their great Activity and Energy; but alſo from the ſudden and ſtrong Effects that ſulphureous Vapours, which are known to deſtroy Elaſticity, are found by experience to have on them. Thus the Fumes of burning Brimſtone will inſtantly deprive all Animals whatever of Life: Thus alſo the ſubtile and moſt penetrating Fumes of fermenting ſpirituous Liquors, are known either to ſtrike thoſe inſtantly dead who ſmell to them, or to infatuate or render Paralytick for Life, thoſe who ſmellto them in leſſer Degrees. Thus alſo the ſulphureous fœtid Fumes

of

of burnt Feathers, *&c.* have an Effect on the disordered Spirits of those who are in Fits. Thus also *Assa Fœtida, Castor*, &c. which abound with a subtile Sulphur, are found to be friendly to the Spirits of the Hysterick; as on the contrary are many Fumes most offensive to the Spirits of others.

29. If the Skin be flead off the Belly of a live Frog, and the *Abdomen* opened on each side, so as that its strait Muscles may by drawing a little on one side, have a strong focal Light cast on the Inside of them; if in this Posture those Muscles be viewed thro' a good Microscope, the parallel Fibres of the Muscles are plain to be seen, with the Blood running alternately up and down, between each Fibre, in capillary Arteries so fine that only single Globule can pass them. If the Muscle happens to act while thus viewed, then the Scene is instantly changed from parallel Fibres, to Series's of *Rhomboidal Pinnulæ* which immediately disappear as soon as the Muscle ceases to act. It is not easy to get a Sight of this most agreeable Scene, because that on the Action of the Muscle, the Object is apt to get out of the *Focus* of the Microscope; but those who are expert in the Use of those Glass-

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es may readily move them accordingly. I have found ſmall Frogs beſt for this Purpoſe, *viz.* ſuch as are not above $\frac{1}{3}$ or $\frac{1}{4}$ of their full Growth. Stimulating the Foot of a Frog, will ſometimes make it contract theſe Muſcles. The Frog muſt be fixed in a proper Frame. If repeated Obſervations were made on the Muſcles thus in Action, it might perhaps give ſome farther Inſight into the Nature of muſcular Motion.

30. It may not be improper here to take notice, that having about twenty ſeven Years ſince, read the unſatisfactory Conjectures of ſeveral, about the Cauſe of muſcular Motion, it occured to me, that by fixing Tubes to the Arteries of live Animals, I might find pretty nearly, whether the Blood by its meer hydraulick Energy, could have a ſufficient Force, by dilating the Fibres of the acting Muſcles, and thereby ſhortning their Lengths, to produce the great Effects of muſcular Motion. And hence it was, as I mentioned in the Preface to the firſt Vol. that I was inſenſibly led on from time to time, into this large Field of ſtatical and other Experiments. Whence we ſee what great Encouragement we have to ſpur us on in theſe Purſuits; ſince the wonder-

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ful Works of the great Author of Nature, are ſo fruitful in furniſhing us, from its inexhauſtible Fund, with freſh Matter for our Reſearches, and thence with the inexpreſſible Delight, of new and farther and farther Motives to adore and praiſe our All-glorious Maker in his Works.

EXPERIMENT X.

1. WE ſee in the foregoing Experiment how very much the Velocity of the Blood is retarded, in its Paſſage from the greater, to the leſſer Arteries in the Guts, notwithſtanding the Sum of the Diameters and Areas of the leſſer do conſiderably exceed thoſe of the greater. So that the Blood in paſſing thro' the Muſcular, the Membranous, and other parts of the Animal, muſt be carried on with innumerable different Degrees of Velocity, and conſequently in different Quantities thro' diſſimilar parts: But in the Lungs eſpecially this Difference is remarkably great, for ſince the whole Blood of the Animal inceſſantly paſſes thro' them; by comparing the Proportion, which the Quantity of the Lungs bears to the reſt of the Body, we may make ſome eſtimate

timate of the great Difference of the Velocity of the Blood in each.

2. I cut the Body of a Spaniel Dog aſunder juſt at his Heart, having firſt taken out the Lungs, the fore part weighed eight Pounds ſix Ounces, the hinder part twelve Pounds eleven Ounces, the Stomach and Guts being waſhed clean weighed one Pound two Ounces

3. I boiled the whole Dog ſo long as to make the Bones eaſily ſeparate from the Fleſh, they weighed two Pounds four Ounces, which being deducted from twenty one Pounds one Ounce, the whole Weight of the Dog; there remains eighteen Pounds thirteen Ounces, for the Weight of the Fleſh, Bowels, Skin, Membranes, *&c.* the Dog was not fat, but allowing ſix Pounds thirteen Ounces for Fat and Hair, thro' which the Blood does not circulate, there then remains twelve Pounds of Subſtance, thro' which the Blood may reaſonably be ſuppoſed to circulate freely.

4. The Windpipe being cut off cloſe to the Lungs, they weighed ſix Ounces two Drams, that is $\frac{1}{30.72}$th part of the twelve Pounds.

5. The whole Blood is inceſſantly circulating, tho' not with equally numerous Revolutions, thro' all the Parts of the twelve Pounds; but

but doubtleſs with vaſtly greater Rapidity thro' the Lungs, than thro' other Parts of the Body.

6. If according to the Eſtimate under Experiment VIII, 4.34 Pounds of Blood paſs thro' the Heart in a Minute, then the ſame Quantity muſt alſo paſs in the ſame time, thro' the Lungs, ſince the left Auricle and Ventricle are ſupplied from thence with Blood.

7. The Sum of the Surface of all the Veſicles of the Lungs of a Calf, were eſtimated *Vol.* I. p. 242. to be equal to 40000 ſquare Inches, whence in proportion to their Weight, thoſe of this Dog muſt be equal to 12121 ſquare Inches: And as there were found in *Exper.* VIII. 4.34 Pounds or 113.684 cubic Inches of Blood to paſs thro' the left Ventricle of that Dog, theſe cubic Inches divided by $\frac{1}{1620}$ part of an Inch, or 0.000672, the Diameter of the fine capillary Veſſels, the Product is 169172 ſquare Inches, which that Quantity of Blood would ſpread into. Theſe divided by 12121, the Number of ſquare Inches in the Veſicles of the Lungs give 13.95, ſo that they are $\frac{1}{13.95}$th part of the expanſe of the Blood: And allowing one half of theſe for the Space between the Cavities of the Blood-veſſels, then the Sum of all the Cavities

of thoſe Veſſels, will be $\frac{1}{27.9}$th part of the whole Expanſe of the 4.34 Pounds of Blood, and conſequently a Quantity of Blood equal to 27.9 times the Capacity of thoſe Veſſels muſt paſs thro' them in a Minute. Whence we ſee by this Calculation, as well as by the above-mentioned ſmall Proportion that the Lungs bear to the reſt of the Body, that the Velocity of the Blood muſt needs be very much accelerated in them.

8. When we view in a ſtrong Light the Blood circulating in the Lungs of a Frog, we ſee the Arteries as they paſs on, ſending Branches, which ſpread like a fine Net-work over the Surface of each Veſicle; and on ſome of theſe Veſicles we may very plainly ſee, the Blood when it has paſs'd over little more than half their Surfaces, to enter correſponding capillary Veins, which thence unite in large Trunks; but on the greateſt part of the Veſicles, the extream capillary Arteries, reach to the Verges of the Veſicles, and there enter at right Angles the Veins, which run along the Limits of thoſe Veſicles; which Veins laying on the inner Sides of thoſe Veſicles they are not viſible like the Arteries: But when in here and there a Place I have clear-

ly ſeen thoſe Veins, I have then alſo ſeen the extream capillary Arteries, pour at right Angles their ſingle Globules, into thoſe much larger Veins; agreeable to what I ſaw in the injected Veſſels in *Exper.* XXI. *Numb.* 8.

9. Now by this means the Blood has a much freer Paſſage thro' the Lungs, where it is requiſite for it to move with much greater Velocity than in other parts of the Body. Whereas in ſome if not all the Muſcles, by entering the finer capillary Veſſels at right Angles its Velocity is much retarded. I have obſerved that where a long capillary Artery branches off at an acute Angle, that there the Velocity of the Blood is many times greater, than where it branches off at right Angles, which plainly ſhews the great Degree of Retardation which the Blood ſuffers by its paſſing off at right Angles. And this Retardation, which was neceſſary in order to prevent the Blood's paſſing too freely, muſt be very conſiderable, where it ſucceſſively paſſes ſeveral times at right Angles, as in the Bowels, Urine and Gall-Bladders and other parts of the Body. On which account, as well as on account of the greater Length of the Arteries, a greater impelling Force was neceſſary

to drive the Blood thro' the great Artery, and its ſeveral Branches than thro' the Lungs; and accordingly the left Ventricle of the Heart is made much ſtronger, thereby to impel the Blood with a greater Force than the right Ventricle does.

10. On comparing the different Velocities of the Blood in the Muſcles, and in the Lungs of a Frog; I found that the Blood moves in the parallel cylindrical capillary Arteries in the ſtrait Muſcles of its Abdomen at the rate of $\frac{1}{10}$th of an Inch in nine Seconds of time, that is at the rate of an Inch in ninety Seconds, or one Minute and a half.

11. But the Blood flowed in the capillary converging Arteries of the Lungs with a much greater Velocity, *viz.* $\frac{1}{10}$th of an Inch in the Time of eight beats of a Watch, which beat 16000 times in an Hour, that is in $\frac{1}{4.795}$th Part of a Second. And there being 345.42 beats in nine Seconds, theſe eight beats of the Watch are $\frac{1}{43.17}$th part of the abovemention'd nine Seconds; ſo that the Velocity of the Blood in the Frog's Lungs was forty three times greater than in its Muſcles.

12. And I obſerved that the Motion of it thro' the Lungs was ſo free, that I could not only

only ſee it ſenſibly accelerated at each *Syſtole* in the fineſt capillary Arteries, but alſo in their correſponding capillary Veins, tho' not in their larger Trunks.

13. And as a Frog's Heart has but one Auricle and Ventricle, the Blood is thrown by the ſame Ventricle, at the ſame inſtant, both into the Lungs and all over the Body; then ſince its Velocity is in Arteries of equal Diameters, forty three times greater in the Lungs than in the Muſcles, notwithſtanding it is impelled, by one common Impetus; this evidently ſhows, that it muſt have proportionably freer Paſſages through the Lungs; and conſequently ſince it has a freer Paſſage, the Heat which it there acquires, by Friction againſt the Coats of the Veſſels, will not be increaſed in proportion to its greater Velocity there, than in other Parts of the Body, but in ſome other intermediate proportion; for as the Blood meets with greater Reſiſtance in paſſing from the Arteries to the Veins in other Parts of the Body, ſo were its Velocity equal in all Parts of the Body, it would acquire the greateſt Heat, where it met with the greateſt Reſiſtance and Friction, which would not in that caſe be in the Lungs: But

as the Velocity of the Blood in the Lungs, is found to be much greater than elſewhere, ſo doubtleſs it there acquires its greateſt Heat; yet in other Parts it acquires ſome, more or leſs in proportion to its different Degrees of Velocity and Friction.

14. I obſerved above *Numb.* 8. that tho' on ſome Veſicles of the Lungs, each extream capillary Artery had a correſponding capillary Vein, into which the Blood paſſes; yet I found that many of the extream capillary Arteries of other Veſicles, emptied themſelves at right Angles, thro' the Sides of much larger Veins; the ſame I alſo found by Injections in *Exper.* XXI. *Numb.* 8. Whence, we may conclude, that though Anatomiſts have juſtly obſerv'd, that the Number of Veins in ſeveral Parts of the Body, are near double to the Number of Arteries, yet this cannot hold true, when we compare the Numbers of the extream capillary Arteries and Veins; for thoſe of the Arteries muſt needs for the abovementioned Reaſons, far exceed thoſe of the Veins in Number.

15. I ſhall hence take occaſion to attempt an Eſtimate, tho' a very inaccurate one, of the Number of extream capillary Arteries in

a Man's Body, in the following manner, *viz.* Suppoſing as is mentioned in *Exper.* VIII. *Numb.* 8. that the Area of the tranſverſe Section of the *Aorta* in Man, is 0.4187 Inch, and that the Length of the Cylinder of Blood, which is thrown out at each *Syſtole* of the left Ventricle, is 3.96 Inches; and the Area of a tranſverſe Section of a fine extream capillary Artery being there ſet at 0.0000298 Inch: Then ſince equal Cylinders are as their Baſes and Heights, the fine capillary Cylinder, equal to the Cylinder thrown out in each *Syſtole* of the Heart, will be 55639.98 Inches long: This Number multiplied by ten gives the Sum of that Column in $\frac{1}{10}$ Inches, *viz.* 55639.98 each of which was the Space the Blood flowed in nine Seconds. But this Length being to paſs the extream capillary Arteries of a Man, in $\frac{1}{75}$th part of a Minute, that is in $\frac{1}{8.88}$ of nine Seconds, (the Time in which the Blood moved $\frac{1}{10}$th Inch in the Frog) tho' not with greater Velocity than in the Frog; therefore the Number of thoſe extream Arteries in Man, muſt be proportionably increaſed by multiplying 556399.8 by 8.88, the Product is 494083, the aſtoniſhing Number of extream capillary Arteries. And if ac-

cording to Dr. *Harvey* and Dr. *Lower*, double that Quantity of Blood be thrown out at each *Syſtole*, then the Number of extream capillary Arteries will alſo be double that Number, *viz.* 988166. And if the Velocity in the Lungs be 27.9 times greater as was found in *Exper.* IX. *Numb.* 6. then the Number of extream capillary Arteries there will be 3541713.

16. And how vaſtly more numerous are the many Branchings, Windings and Turnings of the Arteries and Veins, how innumerable the lymphatic Veſſels, and ſecretory Ducts ? and theſe all adjuſted and ranged, in the moſt exact Symetry and Order, to ſerve the ſeveral Purpoſes of the animal Oeconomy ; *So curiouſly are we wrought, ſo fearfully and wonderfully are we made !*

Experiment XI.

1. As to the Force with which the Blood is impelled from the right Ventricle of the Heart into the pulmonary Artery, it ſeems impracticable to attempt the finding of it, by fixing a Tube to that Artery, in the ſame manner as to the carotid and crural Arteries

teries of living Animals, becauſe the Animal muſt needs dye while it is doing.

2. The Area of the tranſverſe Section of the pulmonary Artery, being in one part, before it divaricates into Branches, of the ſame Dimenſion with the Orifice of the *Aorta*, the Velocity of the Blood in that part, may be accounted the ſame as in the Orifice of the *Aorta*. But tho' the Quantities and Velocities of the Blood in paſſing out of both Ventricles, be the ſame, yet it does not thence follow, that their expulſive Forces muſt be both the ſame: For if the Blood in paſſing into the pulmonary Artery, finds leſs Reſiſtance from the preceding Blood, than the Blood does in entering into the *Aorta*, then a leſs Force will expell it out of the right Ventricle with equal Velocity; and accordingly, as there is not ſo much Force required to drive the Blood thro' the Lungs, as thro' the reſt of the whole Body, ſo we may obſerve, that the Subſtance of the Muſcle of the right Ventricle, has not near the Thickneſs of that of the left. The following Experiments and Obſervations may give us ſome Light into this Matter, *viz.*

3. I fixed a glaſs Tube to the pulmonary Artery of a Calf's Lungs, and then thro' a Tunnel

Tunnel poured warm Water into it; then with a large Pair of Bellows fixed to the Windpipe, I alternately dilated the Lungs, to try if by that means, the Water would paſs into the pulmonary Vein: But I ſoon found myſelf diſappointed, for the Water flowed ſo freely, from the capillary Arteries thro' the Tunicles of the Veſicles, into the Veſicles themſelves, and thence into the *Bronchiæ*, as to flow plentifully thro' the Windpipe, when it hung down in a depending Poſture. At firſt I ſuſpected that the Force of the Water, which was four Feet high, in the Tube affixed to the Artery, might have burſt the thin Blood-veſſels; but I found it the ſame in ſeveral Tryals, on the freſh warm Lungs of Sheep, Oxen and Calves, even when the perpendicular Height of the Water of the Tube was leſs than a Foot; and doubtleſs the Force with which the Blood is thrown into the Lungs, by the right Ventricle of the Heart, is greater than this.

4. And that ſo ſmall a Force of Water could not burſt the Blood-veſſels, I was aſſured by the following Experiment, *viz.* I diſſolved four Ounces of Nitre in a Pint of hot Water, into which Water there flowed from

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the Cut-throat of a Calf, a Quart and a quarter of a Pint of Blood, which was kept in a diluted State by the nitrated Water. Having then fixed a Tube which was two Feet long to the pulmonary Artery of the above-mentioned Calf's Lungs, I poured gradually into the Tube, of the nitrated Blood, as much as the Artery and its Ramifications would contain, which was near a Quart, none paſsing that I could perceive into the pulmonary Vein. The Lungs were much dilated and looked very red: But notwithſtanding the perpendicular Height of the Blood in the Tube was two Feet, yet no Blood paſſed thro' the Tunicles of the Veſicles, into the Veſicles and *Bronchiæ*; for when the Windpipe was held downwards, nothing flowed out but a white Froth; a plain Proof that when the Water was leſs than a Foot perpendicular in Height, in the foregoing Experiments, it did not forcibly break thro' the Blood-veſſels, but muſt paſs thro' Pores which were too fine, for the Globules of nitrated Blood to paſs: Thoſe Pores being perhaps ſomething larger in the Lungs of a dead Animal, than when alive; for upon Death all the Fibres of the Body are relaxed. When I cut a Slaſh into the Subſtance

of

of the Lungs, the nitrated Blood freely flowed out.

5. And that the capillary Arteries were not burſt by the Force of the Water, ſeems farther probable from hence, *viz.* I fixed a Tube five Feet long to the pulmonary Vein of a Hog's Lungs, and poured in warm Water, which neither flowed into the pulmonary Arteries, nor among the *Bronchiæ*, an Argument that this Force did not burſt the Veins, which ſome Anatomiſts ſay, have no Valves in them.

6. When I fixed the ſame Tube to the Windpipe of thoſe Lungs, and poured in Water, it paſſed thro' the *Bronchiæ* and ran out of the Orifice of the pulmonary Artery, but not above one fifth ſo faſt, as when its Courſe was the reverſe, *viz.* from the pulmonary Artery to the *Bronchiæ*, in which caſe it run at the rate of a Pint in a Minute. Yet when Air was blown into the Cavity of the Lungs, thro' the Windpipe, none paſſed thence, either into the pulmonary Artery or Vein.

7. Another time I try'd alſo whether the thin Serum of a Hog's Blood would paſs from the pulmonary Arteries, thro' the correſponding Veins of the Lungs, of the ſame Hog, which

which Lungs were kept warm in Water; the Serum paſſed moſt freely thro' into the *Bronchiæ*, but not into the Veins.

Experiment XII.

1. I Made an Inciſion of two Inches length between the Ribs into the *Thorax* of a Dog, on the right Side: At firſt opening the Lungs were dilated ſo as to fill up the Cavity of the *Thorax*, for they preſſed againſt the inward Orifice of the Wound, and continued in this State for ſome time: But then as this right Lobe of the Lungs fell gradually more and more, ſo the Dog ſhewed more and more Uneaſineſs in breathing; and on dilating or contracting the *Thorax* by the Action of the *Diaphragm*, the Air ruſhed briskly in and out at the Inciſion: But when the Orifice was covered by drawing the Skin over it, the Dog immediately breathed in a natural eaſy manner.

2. Hence we may obſerve, that the Lungs Continuance in a dilated State, for ſome time after the Orifice was made, muſt be owing to the Force of the Blood in the pulmonary Artery, in the ſame manner as the nitrated Blood in *Exper.* XI. *Numb.* 4. dilated thoſe Lungs.

For

For ſince the Subſtance of the Lungs is of a very flaccid Nature, they muſt needs collapſe, when the Air equally preſſed on them both within and without.

3. Hence alſo we ſee that this Dilatation of the Lungs, which ariſes only from the Force of the Blood in the pulmonary Artery, is not alone ſufficient to promote the Paſſage of the Blood thro' them; but there is alſo requiſite a farther Dilatation of the Coats of the Veſicles with inſpired Air; thereby probably to unfold the corrugated Extremities of the Arteries and Veins, and ſo to give the Blood a freer Courſe thro' them. For tho' in Experiment the firſt, it was obſerved that on deep ſighing, that is on much dilating the Lungs, the Force of the Blood in the Horſe's Arteries was greatly increaſed, which was occaſioned by the Blood's flowing more plentifully thro' them when they are dilated than when fallen; yet we cannot hence infer that the Blood flows more freely thro' them alſo when they are thus dilated by the Force of the arterial Blood, without the joint Dilatation alſo by the inſpired Air.

4. But when for want of this Dilatation of the Veſicles by the Air, the Blood's free Courſe

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was in this Dog thus retarded, it muſt needs therefore flow in much leſs Quantity to the left Ventricle of the Heart, which being thus deprived of a ſufficient Supply, the Force of the whole arterial and venal Blood of his Body muſt be proportionably rebated, and conſequently a leſs Supply coming from the Veins, to the right Ventricle of his Heart, the Force of the Blood in the pulmonary Artery, being much abated, ſo as not to be ſufficient to dilate the Lungs, they collapſe : So that if a Tube had at the ſame time been fixed to one of the carotid Arteries of this Dog, no doubt but the Blood would have ſubſided conſiderably in the Tube.

5. But when on ſtraining, the Dog, by the joint Action of all the Muſcles of his *Abdomen*, thereby compreſſed the venal Blood forcibly up into the aſcending *Vena Cava*, the right Ventricle of the Heart being thereby more plentifully ſupplied with Blood, impelled it alſo more forcibly into the pulmonary Artery, ſo as to make the collapſed right Lobe of the Lungs inſtantly to dilate, ſo vigorouſly to puſh the lower part of the Lobe, one, two, and ſometimes three Inches length out thro' the Inciſion ; and that after he had loſt half a

Pint

Pint of Blood. But when more than half of his Blood was evacuated, then the right Lobe did no more dilate on his ſtraining.

6. Hence there does not ſeem to be ſo much danger as has been imagined in the *Paracenteſis* or Inciſion into the Thorax, in order to take away an Abſceſs, *&c.* for tho' while the Orifice was open, this Dog breathed with Difficulty, yet the left Cavity of the Thorax being by means of the *Mediaſtinum* ſtill cloſed, the left Lobe of the Lungs play'd too and fro to ſuch a Degree, that the Dog breathed enough to keep up the Circulation of the Blood for more than a Quarter of an Hour, that I purpoſely try'd: And the Uneaſineſs in breathing, not increaſing in ſo long a time, it is reaſonable to believe that he would have lived in that manner, for ſome Hours. But if the left Cavity of the Thorax had alſo been open at the ſame time, he would doubtleſs have died ſoon. Now ſuppoſing one Cavity of a Man's Thorax to have been open'd, and the Operation that was thereby intended, perform'd; if juſt before the Inciſion be cloſed up, the Man be made to ſtrain, and thereby ſtrongly contract all the Muſcles of his *Abdomen*, the fallen Lobe of the Lungs

will then immediately dilate, upon which the Orifice being inſtantly covered with a Plaiſter, the Man will then breathe as free as ever. *Qu.* Will ſtrongly girding or compreſſing the Abdomen do the ſame thing?

7. But in caſe the Lungs themſelves are pierced with a Sword or Bullet, then this ſtraining muſt needs be hurtful, becauſe it will much increaſe the Effuſion of Blood.

8. Hence we may ſee how dangerous it is for thoſe, whoſe Lungs are ſo weak as to be ſubject to break, to uſe ſtrong Exerciſe or Motion; for when a Man either ſtrains, or is uſing any violent Exerciſe, ſo as either to have the Blood impelled more forcibly into the right Ventricle, or by running, *&c.* to have that Ventricle beat inſtead of ſeventy five times, one hundred and twenty times in a Minute, then the Blood muſt needs be very forcibly impelled into the Lungs.

9. In which caſe the Blood being greatly accumulated in the pulmonary Artery, the Lungs will conſequently be conſiderably dilated, ſo as to ſubſide but little in Expiration; which may be the Cauſe of thoſe ſmall but quick Inſpirations and Expirations, which Men make in ſtrong or quick Motions. This is

what those whose Lungs are considerably wasted or otherwise defective are but too sensible of, even in a small Degree of Motion; for in this case, the natural Course of the Blood thro' the defective Lungs, being much retarded, there is immediately a great Accumalation of Blood in the pulmonary Artery, occasioned by the accelerated Pulses of the Heart: The Capacity of whose right Ventricle, is proportioned to a sound State of the Lungs, as all the Parts of an Animal, both solid and fluid, are in a healthy State, admirably tempered and adapted to each other; but in a defective State of the Lungs, they are too easily surcharged with Blood, so that the unhappy Person is ready to be choaked; that is the Blood does with very great Difficulty and in but small Quantities pass the Lungs, to supply the Demands of the left Ventricle of the Heart, without which Life instantly ceases.

10. There is probably also the like Accumulation of Blood in the pulmonary Artery, in pleuretic Cases, when the Blood by its Siziness does with difficulty pass, tho' impell'd with such Force as to distend the Vessels, and thereby cause pungent Stitches. One reason why the Effects of a sizy Blood are usually

ſooner felt in the Lungs or *Pleura*, than in other parts of the Body, may be this, *viz.* by *Exper.* CXIII. *Vol.* 1ſt, *p.* 253. it was found that when a Gage was fixed to the Thorax of a live Dog, the Spirit of Wine roſe in the Gage, about ſix Inches in ordinary Inſpirations, and twenty or thirty Inches in laborious Inſpirations; whence it is evident that there is then ſo much leſs Preſſure of Air in the *Thorax*, both on the *Pleura* and Surface of the Lungs, the Conſequence of which is, that more Blood will at that time flow into the Veſſels, as they are then leſs compreſſed by Air, as is the known Caſe in Cupping; and which is further evident from the following Experiment, *viz.* I ſlit Holes thro' the intercoſtal Muſcles on each ſide of the *Thorax* of a Kitten, and then placing it under an Air Pump Receiver, it was killed in *Vacuo*; when on opening the *Thorax* I found the Lungs very red with coagulated Blood, which had flowed more freely into its Blood-veſſels, on the Weight of the Atmoſphere being taken off of its Veſicles, both within their Cavities, and on the Sides next the *Thorax*: Whereas if a Kitten be killed in *Vacuo*, without having Orifices made into its *Thorax*,

the Lungs will in this case be very white; for while the Air is drawn away from the Vesicles thro' the *Bronchia*, the Air which is included in the Cavity of the *Thorax*, being dilated compresses the Lungs, and also the Blood out of its Vessels, on which account they are in this case found to be white. And that the Air in the *Thorax*, thus compresses the Lungs in *Vacuo*, is evident hence, *viz.* If the Body of a Kitten, as soon as killed, be cut asunder, just below the Diaphragm, and a Weight be then tyed to the Head, so as to make that obtruncated Part sink under Water, if in this Position it be placed in *Vacuo*, the Diaphragm will dilate considerably and subside again, as soon as Air is let into the Receiver, and it will be the same if not immersed in Water: A plain Proof, that there is Air in the Cavity of the *Thorax*, which while it thus, by its Expansion, dilates the Diaphragm, must needs at the same time compress the Lungs, in which State they are found on opening the *Thorax*: Whereas, Lungs taken out of the *Thorax* do dilate in *Vacuo*, and continue so when Air is let into the Receiver. Another Reason why the ill Effects of such sizy Blood, do probably fall

ſooner on the Lungs than on other Parts, is, becauſe much greater Quantities of Blood, do, in proportion to their Bulk, paſs in equal Times thro' the Lungs, than thro' any other Part of the Body, and conſequently a too viſcid State of it, will ſooneſt be felt there: Or in the *Pleura*, where Anatomiſts obſerve, that the Blood has alſo a very free and ſhort Circuit, by the intercoſtal Arteries, and thence thro' the *Azygos* Vein to the Heart: On which Account there being alſo a very plentiful Flow of Blood into the *Pleura*, the Effects of its Sizeneſs are frequently firſt felt there; and that oftner on the left than the right Side: Probably becauſe the *Aorta* laying on the left Side, the Blood is thence impelled with greater Force, into the ſhorter left, than into the longer right intercoſtal Arteries. Which Inconvenience, as is well known, is often greatly rebated by decreaſing the Quantity of Blood in Bleeding. And thus alſo in plethorick, aſthmatic and other Caſes, the Lungs are often immediately very ſenſibly relieved by abating the Quantity of Blood.

11. The ſtrong cordlike Beatings of a pleuretic Pulſe, has by ſome been attributed to the criſping of the Fibres of the Coats of the Ar-

teries by the pleuretic Heat; but it seems to be more probably owing to the Siziness of the Blood, which on that Account passing with greater Difficulty the capillary Arteries, it must consequently be more accumulated there, by the impelling Force of the Heart. So that if a glass Tube were fixed to the Arteries of a pleuretic Animal, it would probably mount much higher than in a healthy State of the Animal, especially in the beginning of the Distemper, before the vital Strength be impaired.

12. Those whose Business it is to speak long aloud in publick, do very sensibly find the Difference before and after Dinner, in which case it is much more laborious to speak, the Lungs being incommoded with a greater Quantity of Blood then accumulated in the pulmonary Artery.

13. And as it was before observed that the Dilatation of the Lungs, greatly promotes the free Passage of the Blood thro' them, and thereby the more invigorates and exhilarates; so we may observe that those who in speaking in publick, habituate themselves to speak with well dilated Lungs, do it with much more Ease to themselves, and greater Satisfaction to

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the Heaters, than thoſe who draw in but little Breath; who are thereby not only incapacitated from ſpeaking loud, but are alſo at the ſame time greatly incommoded and fatigued for want of a free Paſſage of the Blood through the but little dilated Lungs. This is indeed unavoidable in ſome who labour under the Inconvenience of a narrow *Thorax*; neither are ſuch Perſons uſually ſo healthy, as thoſe who have a large *Thorax* for their Lungs to dilate in, which is commonly with good Reaſon looked on as a prime Symptom of a robuſt and healthy Conſtitution.

14. But the healthy Dilatation of the Lungs, and what is conſequent thereon, the free Paſſage of the Blood thro' them, is much oftner incommoded by Intemperance and Exceſſes in eating and drinking, than by any Defect in the Formation of the *Thorax*. For theſe Exceſſes incommode the Lungs not only by ſurcharging them, with too great Quantities of inflamed Blood, but alſo hinder the free Dilatation of them, by filling the Stomach and Bowels ſo full, as thereby not only greatly to hinder the Action of the Midriff in deſcending, and thereby dilating the *Thorax* and Lungs; but alſo retard the Motion of the

Blood in the Coats of the much diſtended Bowels: Hence habitual Exceſſes, as they uſually occaſion many kinds of Diſorders in other Parts of the Body, ſo do they very often diſtemper the Lungs.

15. And here it may not be improper to obſerve, as it naturally occurs from the foregoing Conſiderations, the great Benefit of Exerciſe even to the temperate Liver; for it not only by the meer Effect of Motion agitates the Blood in all Parts, but alſo gives it a brisk Circulation, not only by its increaſing the Number of the Syſtoles of the Heart, but alſo by giving it a freer Courſe thro' the more dilated and agitated Lungs: Which Dilatation Exerciſe alſo makes more free and eaſy, by promoting Digeſtion and the Deſcent and Evacuation of the Contents of the Bowels, whereby not only the Midriff can more freely act and dilate the *Thorax* and Lungs, but the Blood alſo can have a freer Paſſage thro' the Coats of the Stomach and Guts. Thus in whatever View we conſider the Animal Oeconomy, many cogent Arguments for Temperance and Exerciſe do always occur.

16. When we ſee in *Exper.* XI. *Numb.* 6. how freely the Serum paſſed from the pulmona-

ry Artery into the Cavity of the Veſicles and *Bronchiæ*, 'tis no wonder that ſo great Diſcharges of Humours are often made thro' the ſame Paſſages, when the Blood is much diluted, by being in Colds ſurcharged with too much obſtructed perſpirable Matter, or otherwiſe diſordered: Hence alſo ſome Aſthma's have their Origin.

17. " Sir *John Floyer* in his Treatiſe of the " Aſthma aſſigns the immediate Cauſe of it " to be the Straitneſs, Compreſſion, or Con- " ſtriction of the *Bronchia*. He obſerves that " the Fit of the Aſthma happens ſuddenly, " thro' the Efferveſcence of the Blood, occa- " ſioned by external Cauſes which ſeparate the " *Lympha lactea* from the Blood, which ſtops " in the ſwelled Glands of the Lungs." This Notion of his ſeems to be confirmed by what is found to be conſtantly the Caſe, when Water inſtead of Blood flows into the Arteries of a Dog, in which caſe, as is obſerved in *Experiment* XIV. *Numb.* 5. all the Muſcles of the Dogs were convulſed: And the like Defluxion of thin ſerous Humours on the Nerves and muſcular Fibres of the *Bronchiæ* and Veſicles may probably by contracting them in the ſame manner occaſion the abovementioned Straitneſs and Conſtriction of the *Bron-*

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chiæ in the Fit of the Asthma. This Defluxion of Serum, he says, evidently appears in loose Stools, Fluxes of Urine, great Spiting, Drowsiness of the Head, in the begining of the Asthma Fit.

Experiment XIII.

1. WE see in *Experiment* X. with how much greater Rapidity the Blood passes thro' the Lungs, than thro' any other capillary Vessels of the Body; whence we may with good reason conclude, that it principally acquires its warmth, by the brisk Agitation it there undergoes: This we find by daily Experience, that an accelerated Motion of the Blood by Labour or Exercise, does constantly increase its Heat; whence we may well infer, that it acquires its Warmth chiefly in the Lungs, where it moves with much greater Rapidity, than in any other capillary Vessels of the Body. And that the heat of the Blood arises principally from this Friction, is hence also probable, that on strong or brisk Motion of the Body, its Heat increases much faster, than it could by any effervescent or fermentative Motion. And *e contra*, on a Cessation of the Blood's

Blood's Motion, either when extravaſated, or on Death, the Blood then cools as faſt as any other warm Fluid of equal Denſity, which has no Efferveſcence in it.

2. As fermenting and efferveſcent Mixtures acquire Heat by the brisk Agitation and rubbing of the Particles of the efferveſcent Mixture againſt each other; ſo the Globules of the Blood may well acquire their Warmth, by being briskly agitated, in paſſing with great Velocity through innumerable divaricating and converging fine Canals.

3. And *Qu.* Is not this a principal uſe of the red Globules, which are the moſt compact and firm part of the Blood, and withall elaſtick, whereby they are the more ſuſceptible of Warmth from this Agitation of Friction? Their red Colour intimates their abounding with Sulphur, which makes them the more ſuſceptible and retentive of Heat, than thoſe Bodies which have leſs of it in them: For the more watery a Body is, the leſs ſuſceptible it is of Heat; whence 'tis with good reaſon concluded, that if pure Water were to paſs through the Blood Veſſels, with a Velocity equal to that of the Blood, yet it would thereby acquire no Heat. This is what

we have many inſtances of in effervefcent Mixtures, ſeveral of which will, with ſeemingly equal Degrees of Effervefcence, acquire very different Degrees of Heat, which may depend either on the different Nature or Texture of their component Particles, or the different manner of the Action of thoſe Particles on each other: Thus even of ſolid Bodies rubbed againſt each other ſome will ſooner acquire a burning Heat than others. *Leewenhoeck* has obſerved that the Blood of Fiſhes, which is much cooler than that of other Animals, has a greater Proportion of *Serum* in it: The Blood of a Land Animal has twenty five times more Globules in it than that of a Crab. If according to Dr. *Jurin*'s Eſtimate in *Mott*'s Abridgment of *Philoſ. Tranſact. Part* II. *p.* 143. the Quantity of the Blood Globules be $\frac{1}{4}$th of the Blood: And if as he has eſtimated it a Diameter of a Blood Globule be $\frac{1}{3240}$ of an Inch; then the $\frac{1}{4}$th of the Cube of 3240, *viz.* 8,503,056,000 will be nearly the Number of red Globules in a cubic Inch of Blood: And the Diſtance of the Centres of the Globules from each other will be $\frac{4}{3240}$ Inch.

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4. Dr. *Boerhaave* obſerves that Oil is capable of a much greater Degree of Heat than Water; and that, like the Blood Globules, abounds with Sulphur, which is found ſtrongly to attract Light and Air, which are both very active Principles.

5. Now many ſolid Bodies, which having acquired Warmth by rubbing, being found to be electrical, it put me upon trying whether any, much agitated Fluids, were ſo too.

6. Having therefore put half an Ounce of Mercury into a two Ounce Vial, I ſhook it briskly to and fro for a conſiderable time, and then laying the Vial down Sideways on a Table, I rolled it very gently, ſo as to make the Edge of the Maſs of Mercury approach gradually to innumerable ſmall ſeparate Particles of Mercury which adhered to the Sides of the Vial; where, with Pleaſure I could ſee ſome of thoſe Particles attracted to, and others repelled from the Maſs of Mercury; which plainly ſhewed the electrical Quality it had acquired by Agitation: Yet Mercury heated by an Effervеſcence with double *Aqua fortis* had no Electricity.

7. I poured into a thin *Florence* Flask two Ounces of cold Water, and to that a ſuffi-

cient

cient Quantity of Oil of Vitrol, which made it as hot as my Hand could well bear. Then I approached the bottom of the Flask to ſome Tinſel, Down and Hairs, but it neither attracted nor repelled any of them. And it was the ſame when a ſtrong Efferveſcence was made with double *Aqua-fortis* and Filings of Iron.

8. It was obſervable that theſe ſudden hot Efferveſcenes which aroſe in the Mixture did not ſenſibly affect Tinſel, *&c.* tho' placed near the bottom of the Flask before the Mixture was put in; a probable Argument that no ſubtile Matter ruſhed ſuddenly thro' the Pores of the Glaſs to cauſe that Efferveſcence. Whereas electrical Effluvia do moſt eaſily paſs thro' the Pores of a glaſs Sphere which has been rubbed ſo as to acquire Electricity.

9. Some Tinſel, *&c.* being laid on the Outſide near the Bottom of a Flask, into which ſome Blood had juſt flowed from a Hog, they were not attracted. I placed the Tinſel thus on the Outſide of the Glaſs, thereby to intercept the warm Vapour of the Blood which I apprehended might hinder the Attraction, if the Tinſel had been approached to the Surface of the Blood.

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10. I put two Ounces of the ſame Blood into a Glaſs full of Cells or Cavities made purpoſely to incorporate Oil and Vinegar: ſtopping the Mouth of the Glaſs cloſe, I tied it to a Pole which was ten Feet long, the other end of the Pole being firmly fixed: The Glaſs thus fixed was carried to and fro with long and ſwift Vibrations of the Pole, for ſome Minutes; but the thus agitated Blood which was of a very florid Colour would not attract Tinſel, *&c.* either thro' the Glaſs or when poured out on a Plate.

11. Whereas Blood thus agitated has no Electricity, tho' ſhaken Mercury has, may not this be owing to the watry Part, which abounding much in Blood, as alſo in the above-mentioned effervеſcent Mixtures, does probably check Electricity, tho' not the Heat which is acquired by the mutual rubbing of the effervеſcent Particles againſt each other? Electrical Experiments are found to ſucceed beſt in a dry Air; thus if a glaſs Tube be rubbed to ſuch a Degree as to be ſtrongly electrical, it will inſtantly loſe that Electricity, if moiſten'd with Water either cold or as warm as the Tube: So that we cannot from this Want of Electricity in the Blood, conclude that its Heat

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is not acquired by brisk Agitation and Motion in the Blood-vessels.

12. But we have in the *Muscle-shell* Fish a remarkable Instance of the Electricity of its Blood Globules: For if a Piece of one of the *Bronchiæ* or Gills be cut off and put into a small concave Glass with three or four Drops of its Liquor, and be then placed under a double Microscope, the Blood may be seen greatly agitated in the fine Vessels; and at the cut Edge of the Piece of Gill may with great Pleasure be seen, many Blood Globules repelled from the cut Orifices of the Blood-vessels, and attracted by other adjoining Vessels; also other Globules rolling round their Center, and repelling each other; whence it is plain that Bodies, by brisk rubbing and twirling about may acquire, in a watry Fluid, both attractive and repulsive Vertue or Electricity. If fresh extravasated Blood be placed before a Microscope, the Globules may be seen by mutual Attraction to aggregate and form greater Globules.

13. But tho' it may be doubtful whether the Blood Globules, by reason of the warm watry Fluid in which they float, do acquire an electrical Vertue or no, in passing with great Rapidity and strong Friction thro' the innume-

rable capillary Veſſels of the Body, eſpecially thoſe of the Lungs: Yet as electrical Bodies acquire greater Degrees of Electricity when rubbed in a cold than in a hot Air, ſo it is reaſonable to think that the Globules may acquire conſiderable Degrees of elaſtic Vibrations in paſſing the Lungs: For while by the extraordinary Frictions they there undergo, they are much heated and dilated, they are at the ſame time refrigerated and contracted by the freſh Air that is continually taken into the Lungs: Where, by reaſon of the great Expanſion of the Surfaces of all the Veſicles of the Lungs, a very large Surface of the Blood is expos'd to an equally large Surface of Air contained in thoſe Veſicles, whoſe Coats are ſo extreamly thin, that thoſe two Fluids are ſuppoſed to be $\frac{1}{1000}$th part of an Inch within Contact of each other, ſo that like blended Liquors, they muſt needs have a conſiderable Effect on one another, the Air in cooling the Blood, and the Blood in warming the Air.

14. Which Effect of the Blood on the Air in the Lungs is ſo conſiderable, that tho' that Air is by Inſpirations blended with a good Quantity of freſh cool Air, no leſs than 1200 times in an Hour; yet by holding the Ball

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of my Sp. v. Thermometer in my Mouth for a considerable time, while I breathed warm Air on it, but drew in the cool Air in Inspiration thro' my Nostrils, the Sp. v. rose in the Thermometer from ten Degrees, the Heat of the external Air, to forty six Degrees above the freezing Point; so that in the $\frac{1}{1200}$th Part of an Hour, or 3″ of a Minute the inspired Air had acquired thirty six Degrees of Heat. The natural State of the Blood, in which State it then was, being sixty four Degrees, and that of the external Air ten Deg. it was therefore fifty four Degrees colder than the Blood, yet in so short a time it acquired thirty six Deg. of Heat.

15. The Quantity of Blood, which passes thro' the Lungs of a Man in a Minute, being estimated in *Experiment* VIII. *Numb.* 12. to be 8.74 Pounds or 228.8 cubic Inches; and the Quantity of Air drawn in at each Inspiration being forty cubic Inches, as estimated in Vol. I. p. 243, they will amount to eight hundred cubic Inches in the twenty Inspirations of a Minute; so that this Quantity of Air will be to that of Blood as 3.48 is to 1: The Blood's specific Gravity to that of Air is as 841 to 1.

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16. I communicated theſe *Data* to Dr. *Deſaguliers*, who together with Mr. *Ch. de Labely*, who was then preſent, both agreed in the following Calculation of the Degree of Refrigeration which the Blood received from the inſpired Air, *viz.*

17. Actual, is to ſenſible Heat, as felt by the Hand or ſhewn by a Thermometer, as Momentum is to Velocity.

18. Senſible Heat multiplied by the Quantity of Matter, gives actual Heat or Momentum of Heat.

19. Therefore actual Heat divided by Matter gives ſenſible Heat, as Momentum divided by Matter gives Velocity.

20. Therefore as we increaſe the Matter, we decreaſe the ſenſible Heat.

21. What gives ſixty four Degrees of ſenſible Heat to one, gives but one Degree of ſenſible Heat to ſixty four.

22. The ſpecific Gravity of Blood to that of Air, being as 841 to 1, if a Bulk of Air, which is to a Bulk of Blood, as 3.48 to 1, be ſo condenſed as to be reduced to the Bulk 1, or the ſame Bulk as the Blood, it muſt be brought to a ſpecific Gravity ſo much greater, than the Diſproportion or ſpecific Gravity of

Blood to Air will be 241.6 to 1, becauſe $\frac{841}{3.48}$ $=241.6$.

23. The Queſtion is reduced to this, *viz.* what gives thirty ſix Degrees of ſenſible Heat to $\frac{1}{241.6}$, how much will it give to one?

24. The Anſwer is $36 \times \frac{1}{241.6} = \frac{36}{241.6} = 0.149$ or about 149th of a Degree.

25. Now as $3''$ 0.149 :: 60 :: 2.98, therefore in one Minute the Heat added to the Blood in the Lungs will be 2.98 Degrees, the whole Heat of the Blood there being $64 + 2.98$ Degrees $=66.98$.

26. So that if a Man holds his Breath one Minute, the Blood in the Lungs from ſixty four Degrees of Heat will be increaſed to 66.98, and in two Minutes (during which time ſeveral Men hold their Breath or can blow without ceaſing, as *Grano* the Trumpeter can) the Heat will be got up to 69.96.

27. But then upon the Mixture of that warmer Blood with the reſt of the Maſs of Blood, the ſenſible Heat muſt be diminiſhed; for that which in two Minutes, gives to the Quantity of Blood in the Lungs the ſenſible Heat of 5.96, will give as much leſs ſenſible Heat to the whole Maſs of Blood, as the Quantity of the whole Maſs is greater than one, which

which was eſteemed the Quantity of Blood in the Lungs.

28. Calling therefore the whole Quantity of Blood x, or the Proportion of the whole Quantity of Blood in the Lungs, Ratio $x:1$; $x:1::5:96$ (Degrees of ſenſible Heat in the Lungs acquired in two Minutes) to the Degrees of ſenſible Heat of all the Maſs of Blood acquired in the ſame time, that is $\frac{5.96}{x}$. This Quantity muſt be added to the Heat of the whole Blood for every two Minutes, if the Breathing be ſtopped ſo long.

29. Now the whole Quantity of Blood in a Man being eſtimated twenty five Pounds: The Quantity of Air forty cubic Inches $=1.24$ Pounds, that is, ſuppoſing its ſpecific Gravity to be equal to that of the Blood; but the Bulk of the Blood has been found above to bear a Proportion to the Bulk of Air in the Lungs as 1 to 3.48, therefore if we ſay as 3.48 to 1. ſo the Weight 1.24 Pounds (of the Bulk of Blood equal to that of Air in the Lungs) is to the Weight of the real Quantity of the Blood in the Lungs $=\frac{1.24}{3.48}=0.356$ of a Pound; conſequently multiplying the Heat acquired in two Minutes 5.96 Degrees into the Quantity of Blood, it is in 0.356 of a

Pound we have the whole additional actual Heat $=5.96 \times 0.356 = 2.12176$, which being divided by the whole Maſs or Quantity of Blood $= 25$ Pounds gives $\frac{2.12176}{25} = 08487$ of a Degree. So that the whole Maſs of Blood, by avoiding Inſpiration for two Minutes, will be increaſed in Heat from ſixty four Degrees to 64.08487 Degrees.

30. And if the Heat of the Blood increaſes as the Times, then in half an Hour the whole Maſs would be increaſed in Heat from ſixty four Degrees to $64 + 1.27305 = 65.27305$ Degrees.

31. Dr. *Boerhaave* relates the remarkable ill Effects of breathing in a very hot Air. For having cauſed a Sparrow to be put into a Sugar Baker's drying Stove, the Heat of which was ſo great as to raiſe the Mercury in *Fahrenheit*'s Thermometer to a hundred and forty ſix Degrees; that is 0.78 more than the natural Heat of the Blood, *viz.* ninety two; the Sparrow after about a Minute expreſſed great Uneaſineſs and dyed in ſeven Minutes. A Cat alſo put into the ſame Stove expreſſed great Uneaſineſs in a Minute and dyed in about ſixteen Minutes, it was as wet with Sweat as if dipped in Water; but a Dog which was put

in

in at the ſame time did not ſweat; after ſeven Minutes he panted much for Breath, and in a Quarter of an Hour expreſſed very great Uneaſineſs, ſoon after which he grew faint and died in twenty eight Minutes: He drivelled all the time a great Quantity of red Foam, which did ſtink ſo intolerably, that a ſtrong labouring Man who went near it, was almoſt ſtruck down in an inſtant with this Stench.

32. He obſerves in this Experiment the dire Effects of this Degree of Heat, how ſoon it brought on a moſt acute Diſtemper, with very violent and mortal Symptoms: How ſuddenly the Humours were changed, from a healthy to a nauſeous putrid State, more peſtilential and deadly than the rotteneſt Carcaſe, how greatly the Humours muſt be altered in ſo ſhort a time, to make the *Saliva* red. He alſo juſtly obſerves that theſe were not the mere Effects of the Heat of the Stove; for if the Fleſh of a dead Animal had been hung up there, it would have dried, and not have turned to peſtilential Corruption; which muſt therefore ariſe from the Friction cauſed by the vital Motion of the Blood in the Lungs; where it being in this caſe not at all refrigerated, did thereby acquire a greater Heat

than that of the Stove; whence its sudden Tendency to Putrefaction, the Oils, Salts and Spirits of the Dog being thoroughly putrified in twenty eight Minutes.

33. He observes also that when a Man breathes an Air as hot as his natural Heat, he soon finds such a Difficulty of Breathing, that he cannot long endure it, but earnestly pants after cooler Air, which invigorates, while hot Air weakens and dispirits; for neither Animals nor Plants can long bear a hot Air, without Intervals of cool refreshing Air.

34. Whence he justly infers, that as the Blood is most heated in the Lungs, by reason of the great Velocity and Friction it there undergoes, so it is there also most refrigerated. *Elementa Chemiæ Tom.* I. *p.* 275 to 278.

35. And *Tom.* II. *p.* 378, he observes that the natural Heat of the Blood is not far from the coagulating Point, which is a hundred Degrees, and the natural Heat ninety two: Whence it may be concluded that a Fever Heat must tend to coagulate the Blood; in order to resist which Tendency, Nature is then under a Necessity of greatly accelerating its Motion thro' the circulatory Vessels,

which,

which, as it promotes Attenuation, does alſo at the ſame time increaſe Heat.

36. As the natural Heat of the Blood is not far from the coagulating Degree, to which and a much greater Degree, it does we ſee ſoon riſe, if not frequently refrigerated, by the Inſpiration of freſh Air: So this makes it very probable that one conſiderable Uſe of the Lungs, is to refrigerate the Blood; Attenuation and ſeparating the Blood Globules is alſo doubtleſs another great uſe of them: For tho' the Blood Globules are ſeparated and paſs ſingle, from the innumerable extream capillary Arteries of the reſt of the Body, to the correſponding Veins, yet the venal Blood is not florid, which Floridneſs may in a good Meaſure be owing to the ſtrong Agitation, Friction and Comminution which it undergoes in paſſing, with much greater Velocity thro' the Lungs than other parts of the Body; in like manner as in *Exper.* XIII. *Numb.* 10. the Blood which was much agitated in a cloſed Glaſs Veſſel, was obſerved to be very florid, not only on its Surface but thro' its whole Subſtance as arterial Blood is. 'Tis probable alſo that the Blood may in the Lungs receive ſome other important Influences from the

the Air, which is in ſuch great Quantities inſpired into them. It has long been the Subject of Enquiry of many, to find of what uſe it is in Reſpiration, which tho' it may in ſome Reſpects be known, yet it muſt be confeſſed that we are ſtill much in the dark about it.

37. As the Air in ordinary Inſpirations and Expirations, paſſes freely and eaſily too and fro with ſmall Velocity, ſo it cannot have any conſiderable Effect on the Blood by any impulſive Force: Nor yet by any additional Gravity ariſing from the Form of the Lungs, which Gravity, upon a Calculation that the Sum of all the Areas of the Veſicles is equal to a hundred and fifty two ſquare Feet, has by Dr. *James Keil* been eſtimated to be equal to 50443 Pounds Weight, there being a manifeſt Miſtake in the Argument. For ſuppoſe a cubic Foot of any ſolid or fluid Matter, to be divided into an hundred *Lamina* or Plates, each of theſe Plates when ſpread abroad will be preſſed with the whole Weight of the Atmoſphere; and when laid on each other in the Form of a cubic Foot, they will then alſo each of them ſuſtain the ſame Weight of the Atmoſphere; beſides that in this Poſition, all

except

except the uppermoſt, will have the additional Weight of the Plates which lay on them; whence it appears, that the Blood will have leſs Weight on it, when ſpread into a broad thin Expanſe, than when in a larger Maſs.

38. As the Blood acquires different Degrees of Warmth according to the different Degrees of Velocity with which it is carried on, and alſo in proportion to the different Diameters, Laxity or Tenſeneſs of the Veſſels; hence in a more lax State of the Fibres of the Veſſels, the Blood will be cooler, more viſcid, and leſs florid and digeſted: But *vice verſa* in a more firm and tenſe State of the Coats of the Blood-veſſels the Warmth of the Blood will be the greater, it being in thoſe more hale vigorous Conſtitutions, impelled more briskly thro' the tenſer capillary Veſſels; whence a conſtant greater Warmth and Vigour in theſe Conſtitutions, and conſequently a more highly digeſted and attenuated Blood. But when the Heat riſes to the Degree of a Fever, then it often putrifies the Blood.

39. Tho' we cannot on the one hand reaſonably ſuppoſe, that there is a ſtrong repelling Degree of Ferment or Effervеſcence in the Blood, in its healthy State, ſo neither on the other

other hand can it be imagined to be a meer languid inert Fluid: For it muſt needs be that the Parts of a Fluid, which is ſtored with ſuch active Principles, will be in a vibrating State, while actuated with ſo conſiderable Degrees of Friction and Heat, as the Blood is: Which Vibrations are reſtrained within due Bounds, by the attractive Power of the Sulphur, which abounds in the Blood to ſuch a Degree, that tho' great Quantities of fermented Liquors are daily taken in, and mixed with the Blood, yet they are thereby ſo reſtrained, as not to be able to raiſe the Blood, into a ſtrongly repelling Degree of Ferment; tho' they much increaſe its Efferveſcence and Heat. When theſe Liquors are taken in to an intemperate Degree, they do then raiſe the Efferveſcence of the Blood, to ſuch a feveriſh Heat, as requires many Hours time, before it be abated, and the Blood brought to a right Temper again.

40. When we conſider that all vegetable Ferments are chiefly carried on by the Action and Reaction between the Air and ſulphurous Particles; and alſo that theſe Principles, with which the Blood is ſtored, do in a fixt State, form Tartar in the Urine; and at the ſame

time

time remember, that it is obſerved by Phyſicians, as an Indication that the Fever abates, when the Urine depoſites a rediſh Brick coloured Sediment, that is, a Tartar; have we not reaſon to ſuſpect that this Tartar, while it was in an elaſtic State in the Blood, greatly contributed to its feveriſh Heat; which Heat therefore abates in proportion as theſe active Principles are ſubdued and reduced to a fixed State, fit to be carried off by Urine or other Evacuations.

41. The right healthy State of the Blood muſt conſiſt in a due *Equilibrium* between theſe active Principles, ſo as not to have them too much depreſſed and fixed on the one hand, which might tend to an acid Acrimony, nor too much raiſed and exalted on the other hand, which makes it tend to an alkaline Acrimony. When we conſider therefore by what an innumerable Combination of Cauſes this *Equilibrium* is liable to be diſturbed, we cannot wonder that our Health is ſo often interrupted, and the Period of Life moſt uncertain as to its Duration. It is wonderful that the fixt organical Parts of our Bodies, which are of a moſt curious and delicate Texture, ſhould hold it out ſo long, without being diſ-

ordered or wearing out; but the Wonder is greatly heightened, when we conſider for how many Years Continuance, the nice healthy Ballance of Power, between the active Principles of the Blood is maintained, notwithſtanding the many rude Shocks it meets with, either from unkindly Food, or inclement Seaſons, and above all from Intemperance.

42. When the morbid Blood is ſo groſs or viſcid as not eaſily to paſs the finer capillary Veſſels, it being thereby greatly retarded in its Motion, cauſes the cold Fit which uſually preceeds a Fever or Ague. And as warm Liquors, ſuch as Urine, *&c.* are obſerved to turn from clear to turbid and to depoſite Sediment as they cool, and when warmed again to reſorb that Sediment and become clear; ſo 'tis probable that as the Blood grows cool on the firſt coming on of the Fit, that Coolneſs may be greatly increaſed, by the then more thick turbid State of the Blood; its Motion being thereby proportionably retarded: But when after ſome time the retarded Blood, which is at this time, for want of a free Paſſage, probably accumulated in the Arteries, ſo as to be at length forcibly impelled thro' the capillary Veſſels, then by the greater Friction

tion of its groſs Parts it acquires a burning Heat; which Heat is prolonged to different Periods of Time in proportion to the Quantity of the groſs morbific Matter, till it is at length either ſufficiently attenuated by repeated Circulations and Diluters, or elſe ends in Death.

43. If, as was before obſerved, the Blood grows more thick and turbid as it cools, it may then be owing to this, that bleeding and purging, which as they greatly cool may alſo thereby bring on an Ague Fit, which is well known to be the Conſequence of thoſe Evacuations, when the Blood is at any time in an aguiſh Diſpoſition.

44. A too relaxed State of the capillary Veſſels may in an aguiſh Conſtitution greatly contribute to the Return of the Fits, for the Blood acquiring by this means, in a determinate time, too great a Viſcidity, the next periodical Fit may thereby be brought on.

45. The groſs tartarine gouty Particles are apteſt to ſettle and cauſe inflammatory Obſtructions in the Extremities of the Body as in the Feet and Hands, where the progreſſive Force of the Blood is leaſt, as being fartheſt from the Heart. And when theſe Humours fix in the Trunck of the Body, they are apter

to ſettle in the Stomach than in the Guts; becauſe in the former they have as much greater a length of capillary Veſſels to retard the Motion of the Blood, as half the Circumference of the Stomach is greater than that of the Guts; for the Arteries of the Stomach do not enter its Coats on one ſide only, as thoſe of the Guts do, but it is ſupplied with Blood by Arteries which enter, ſome on the upper and ſome on its lower ſide; their converging Branches inoſculating about the middle of the Sides: Without this neceſſary Precaution, the Motion of the Blood muſt needs have been there greatly retarded, if it had entered its Coats only at the top or bottom; becauſe in that caſe it muſt have paſſed thro' capillary Veſſels of double the Length that they now are.

46. When any groſs Matter from an Ulcer returns into the Courſe of Circulation, it firſt cauſes a Shivering by its Obſtructions; but when this Matter is impelled by the Force of the circulating Blood thro' the fine capillary Veſſels, it then frequently raiſes a feveriſh Heat by its greater Friction in thoſe Veſſels.

47. In dropſical Caſes when the Blood is poor and watry the Patient complains of great Degrees of Cold, the Blood being very defective in a ſufficient Quantity of red Globules to give it Warmth. Which yet at Intervals will be raiſed to a feveriſh Heat, for want of a due Quantity of thin Serum, and by the Return of ſome of the rancid extravaſated Humour into the Courſe of Circulation.

48. Thus alſo when too much Blood has been evacuated, it is long before a Man recovers that Loſs, and he is too cool, not only for want of a ſufficient Quantity of Blood to be impelled with Vigour thro' the capillary Veſſels, where, as we have before ſeen, it meets with much Reſiſtance, but principally thro' too great a Deficiency of red Globules, whereby to procure it a ſufficient Degree of Warmth, as alſo thereby to keep the Serum diluted by their innumerable runnings to and fro: For if Quantity of Fluid alone would make good the Defect, there would be a ſufficient Quantity of Fluids in the Arteries and Veins, ſome time after each Meal; but this alone will not compenſate for the Loſs of Blood. Beſides, when the Serum of the Blood is too thin, the Globules are much apter to coaleſce, for the thin-

ner the Fluid the more readily will mutually attracting Particles, which ſwim in it, coaleſce. But on the contrary too great a Proportion of Globules diſpoſes the Blood to be inflammatory.

EXPERIMENT XIV.

1. WHEN I had ſeen to what Height the Blood would riſe in Tubes fixed to the carotid Arteries of ſeveral Dogs, then taking away that Tube, I immediately affixed to the Pipe that was inſerted into the Artery another Tube, which was four Feet and half high, to the middle of the Tunnel which was fixed on the top of it: Then cutting open both the jugular Veins, blood warm Water was poured into the Tunnel, which flowing down thence from a perpendicular Height equal to that of the arterial Blood in the former Tube, was by this means impell'd thro' all the Arteries of the Body with a Force nearly equal to that with which the Blood was propelled by the Heart, and was thence carried with the venal Blood out at the Jugulars; where the flowing Blood became more and more diluted with Water, till the Dog died; after which very little Water would paſs out at the jugular Veins. When the Column of Water in the Tube was $9+\frac{1}{2}$ Feet

Feet high, the Blood then flowed more briskly out at the Jugulars.

2. The Dogs conſtantly died when the Blood grew very dilute with Water; whence we ſee the meer keeping the Arteries full with any Fluid will not ſupport Life: No wonder then that the Lamp of Life burns more and more dimly, and is in further and greater danger of being extinguiſhed, in proportion as that noble vital Fluid the Blood, becomes more and more depraved.

3. It was obſervable that the Dogs conſtantly expreſſed great Uneaſineſs inſtantly as ſoon as the blood warm Water entered the Arteries, and mixed with the Blood. Hence if our Drink entered the Blood-veſſels at once it would be moſt pernicious; but Nature gradually aſſimilates it, by firſt mixing it with the Chyle, and other Juices, which are in great Plenty ſecreted from the Glands of the Stomach and Guts, as alſo from the Liver and Pancreas, and from the meſenteric Glands; which Commixture and Aſſimilation is inceſſantly carrying on by infinite Mixtures in the circulating Veſſels, without which the Blood could not be diluted, but its ſtrongly attracting Globules would ſoon coaleſce.

4. This warm Water, thus mixed with the Blood, did uſually make the Dogs vomit, eſpecially when the Column of Water was $9+\frac{1}{2}$ Feet high; whence we ſee that warm Water in the Blood has the ſame Effect, in giving a convulſive Motion to the muſcular Fibres of the Stomach, as when taken inwardly, in which caſe it is well known to cauſe a *Nauſea* and Vomiting; a probable Argument that ſome of it then ſoaks immediately out of the Cavity of the Stomach among its muſcular Fibres.

5. And it has the like Effect upon all the Muſcles of the Body; for when the Water enters the Muſcles two or three Minutes after the Dog is manifeſtly dead, then all his Muſcles are in ſtrong Convulſions, and that for ſome Minutes.

6. If the warm Water was continued thus flowing into the Artery for half an Hour, or two Hours, all the Parts of the Body would during that time, be continually ſwelling bigger and bigger, ſo that there would be a univerſal Dropſy over the whole Body; both the *Aſcites* and the *Anaſarca*: The ſalival and other Glands were greatly ſwell'd, and the Mouth and Noſe filled with muſcoſe ſlimy Matter

which

which flowed from thoſe Glands; the *Ubera* were much diſtended by the filling of their fat Veſicles, as were alſo all the fatty Veſicles of the Body. All the Muſcles were ſwelled and the Interſtices of their Fibres filled with Water; and ſome of them were by this means waſhed white. All this was effected with a Force of Water no greater than that of the arterial Blood in its natural State.

7. Whence 'tis probable that no Veſſels were broken to make way for this univerſa Inundation; but the penetrating Water might readily paſs thro' Pores, and ſuch fine ſecretory Ducts, as the more viſcid Part of the Blood in its common Courſe of Circulation never enters; but thro' which its more attenuated and diluted Parts do in their due Proportion paſs. Thus for inſtance we ſee when the penetrating Water flows freely into the ſecretory Ducts of the ſalivary Glands, it makes the *Saliva* flow plentifully thro' the excretory Ducts, which in a natural State being more gradually ſeparated from the Blood flows alſo more leiſurely thro' thoſe Ducts.

8. But when the Tube, thro' which the Water flowed into the Arteries, was 9$\frac{1}{2}$ Feet high, then ſuch a Force would ſometimes

drive ſome little Blood thro' the ſalival Glands, and among the fat Veſicles of ſeveral Parts of the Body, as alſo into the Cavity of the Guts; which would thereby be diſtended full of Water, if the *Abdomen* was open; but when that was not opened, tho' Water continued for a hundred Minutes to flow in at the carotid Artery, ſo that all the Parts of the Body were exceedingly ſwelled, and there was Water alſo in the Cavity of the *Abdomen*; yet there was not half ſo much Water in the Cavity of the Stomach and Guts, as there would have been, if the *Abdomen* had been all that time open. Hence we ſee how the great Compreſſion of dropſical Humours on the Blood-veſſels and ſecretory Ducts may retard the natural Secretions of Humours into the Bowels and thro' other Glands; whereby the Blood muſt needs become more and more depauperated, for want of a ſufficient Supply of thoſe Secretions, to mix with, and aſſimilate the Chyle gradually into Blood. Hence alſo from the Stoppage of the Secretions of the ſalival Glands, the conſtant Thirſt of the Hydropic.

9. Frequently, when the Dog was dead by waſhing his Blood out in the manner de-

ſcribed,

ſcribed, *Numb.* 1. of this Experiment, I uſed then while all was warm, immediately to open his Abdomen and Thorax, and then fix the Tube to the deſcending *Aorta* a little below his Heart: This large Artery admitting a braſs Pipe of a larger bore, the Water flowed more freely in; which it continued to do for more or leſs time, according to what Experiments and Obſervations I intended to make. And while the Water thus flowed thro' the Arteries, I kept all parts of the Dog warm, by pouring Water on him, and then laying a Cloth dipped in warm Water over his Body, and ſometimes by placing him in a Veſſel of Water.

10. Tho' this Preſſure of Water was equal to the Force of the arterial Blood, which had been waſhed out in killing the Dog, yet none of the warm Water paſſed thro' the Kidnies into the Ureters and Bladder, tho' the Kidnies were diſtended to Hardneſs: Nor would any paſs thro' the Blood-veſſels of the Bladder into its Cavity, whence 'tis probable that it has no Lymphaticks at leaſt within ſide: Yet the Blood-veſſels of the Coats of the Bladder were filled with Water, as is evident, becauſe they were conſtantly on Injection well filled with the tinged Liquor. A probable Argument

that no Urine is thence ſeparated immediately from the Blood into the Bladder; and conſequently there can be no other Paſſage for the Urine into the Bladder than thro' the Kidnies and Ureters: The eaſy Paſſage of Fluids from the Stomach and Guts, immediately into the meſenteric Veins, and the great Velocity of the Blood in circulating, may rationally enough account for the quick Effects, that ſome Fluids have on the Urine ſoon after drinking.

11. The Liver from its own natural Redneſs and Degree of Firmneſs, turned gradually leſs red, and ſometimes whitiſh, but always ſwelled and was very hard; the Water did not flow thro' its Veſſels into the *Vena Cava*: The Gall Bladder was conſtantly diſtended very full, ſo as to flow into the Guts. The Pancreas was full of Water as alſo the Spleen, which was ſeldom much diſtended, but was ſometimes waſhed ſo clear of Blood as to be very well injected with tinged Liquors.

12. I ſlit open four or ſix Inches Length of a Gut, juſt oppoſite to the Inſertion of the meſenteric Veſſels into the Gut: And having then ſo folded open the Gut as to have the inward mucoſe Coat outwards, I wiped it dry

with a Spunge, which was ſoon moiſten'd in every part again, by the Water which ouzed thro' from the meſenteric Arteries; and it was the ſame on frequent Repetition, and alſo when I held this open part ſo as to make a ſmall Cavity or Baſin with it, it would then alſo be ſoon filled with Water.

13. In another Dog whoſe Gut was not ſlit, the Quantity of Water which flowed in, was in ſome Continuance of time ſo great as to diſtend the Guts and even burſt the Stomach. Whence we ſee how eaſily the thinner Part of the Blood may flow into the Cavity of the Stomach and Guts, as it is found in Fact to do in living Animals: And as ſuch large Quantities are thus ſecerned into the Cavity of the *Viſcera*, this muſt needs have a great Influence on the Blood, when theſe Secretions are either too plentiful and thereby diſorder it by too great Evacuations, or when they are on the other hand too ſmall, or are too ſuddenly ſtopped, the ill Effects of which are ſoon felt in the Head and Lungs, whence often a Fever.

14. It is common for habitual hard Drinkers to labour under the Inconvenience of too great Diſcharges of theſe Humours into their Stomachs,

Stomachs, which is occasioned by their continually swallowing down too great Quantities of Liquor, with which the Blood being surcharged, must needs make those more than natural Discharges of it into the Stomach, which they usually complain most of in a Morning. Whence readily inferring that they have a cold watry Stomach, they seldom fail of warming it again soon, by so agreeable a Prescription, as a large Dose of the same Liquor; sagaciously concluding with the poor unhappy *Minute Philosophers*, that because it is most agreeable to depraved Nature, therefore it must be best; tho' in reality it does necessarily increase the Distemper.

15. While the Tube was thus fixed to the descending *Aorta*, and Water continued flowing thro' it, I cut the *Vena Porta* asunder, which carries the Blood from the Stomach and Guts thro' the mesenteric Veins to the Liver: The much diluted Blood which was contained in it, for want of a free Passage thro' the Liver, rushed forcibly out of the Vein; but afterwards the Water which came from the mesenteric Arteries, flowed out of the *Vena Porta*, at the rate only of half a cubic Inch in forty Seconds of Time, for

want of a free Passage from the Arteries to the Veins.

16. When I fixed the abovementioned Tube to the *Vena Porta* of another Dog, so as to make the Water pass thro' it to the Guts, having then slit open a part of a Gut as before describ'd, *Numb.* 12. I found the Water ouzed plentifully thro' the whole mucose Coat into the Gut; whence we see there is a Passage for the Chyle to pass immediately from the Cavity of the Guts into the mesenteric Veins.

17. Yet when *e contra* a Tube was fixed to the Cavity of a Gut and warm Water poured in, no Water would pass thence into those Veins, tho' the Column of Water which pressed to enter, was of several Lengths from one to $9+\frac{1}{2}$ Feet high; the Entrance of the Water being hindred by the connivant Valves which cover the Orifices of those capillary Veins, which opening obliquely into the Intestines, their Orifices are compressed by the incumbent Water: Were it not for this wise Precaution, very gross and noxious Particles of the Contents of the Bowels would have been impelled thro' these Veins and the lacteal Vessels, into the Habit of the Body, and that in greatest Abundance, when the Guts were most

most distended either with Ailments or Wind. Now the Force of the Blood in the Veins being not above $\frac{1}{10}$th or $\frac{1}{12}$th of that of the Arteries, and their Number and Capacities being also much greater, they are thereby more aptly disposed to imbibe Chile from the Intestines, whose peristaltic Motion, together with the alternate Dilatations of the Arteries, and reciprocal Compressions, Dilatations and Relaxations of the Diaphragm and abdominal Muscles do greatly promote its Progress. But when thro' any Obstructions the free Course of the Blood thro' the Liver is retarded, it being necessarily thereby more accumulated in the mesenteric Veins and *Vena Porta*; the imbibing Power of those Veins from the Intestines will not only be proportionably rebated; but also the Velocity with which the Blood should pass along the mesenteric Arteries, thro' the Coat of the Guts, being thereby retarded, will subject the Bowels to many Disorders.

18. It seems evident from this XIVth Experiment, that the Secretions, which are different according to the different Texture of the secretory Vessels, and which are made from the arterial Blood thro' Vessels finer than the

finest

finest circulating Arteries, are not made *pleno gurgite*, that is, with the full Force of the arterial Blood; for if they were, then all the secretory Vessels and Glands would swell as they do in this Experiment with Water; as they do also in dropsical Cases, when the redundant thin Fluid is separated too freely from the Blood. These Secretions must therefore be made more gradually and sparingly, so as to be carried forward in those very fine Vessels, by an alternate pulsive Force of the arterial Fluid, and attractive Power of the fine secerning Vessels; assisted also by constant Vibrations, for the animal Fluids and Solids are in an incessant mutually vibrating State. In this manner doubtless the plentiful Secretions are made into the Stomach and Guts, as also in the Pancreas, mesenteric, salival and other Glands of the Body. And thus also the perspiring Matter is carried off, not alone by the meer protrusive Force of the arterial Fluid, but also by the Warmth and mutually vibrating Action of the Fluids and Solids. And when by Labour or other brisk Exercise, the Velocity of the Blood is increased, and consequently its Warmth, then not only its Force, but the Vibrations also of the Fluids

and

and Solids being thereby increaſed, the Perſpiration is conſequently alſo ſo much increaſed, as to paſs in the viſible Form of Sweat thro' the Pores, which by *Experiment* XV, *Numb.* 9. are dilated by Heat.

Experiment XV.

1. THIS hydraulic and hydroſtatical Method of examining the animal Canals, is not only of uſe to ſhew the Force of the Blood in the Veins and Arteries, the great Reſiſtance it meets with in its Progreſs thro' the finer capillary Veſſels, and many other things which are ſhewn and deduced from the foregoing Experiments: But may alſo be very ſerviceable in ſhewing what Effect different Liquors have on the finer Veſſels of the Body, *viz.* when they are either hot or cold, or have different Degrees of reſtringent or other Qualities.

2. For ſince the healthy State of an Animal conſiſts principally in a due Equilibrium between its Fluids and Solids, ſo that an Error in the Solids will greatly contribute to the diſordering of the Fluids too, it will therefore be very uſeful to ſee, what Effects different

ent Liquors have on thoſe Solids, either in relaxing them when too tenſe, or in contracting and ſtrengthening them when too lax. And tho' theſe ſeveral Properties of many of them are by long Experience well known to the skilful Phyſician, yet ſince it is making ſome Advance in Knowledge, farther to illuſtrate even known Truths, a fuller Demonſtration of the Effects of ſome of theſe Liquors may be of Service in confirming the known Explications of the Operations of Medicines, and in farther clearing up the Reaſons of ſuch as are more doubtful.

3. I took a young Spaniel Dog which weighed twenty one Pounds, and as ſoon as he had bled to Death by having his jugular Veins cut, I immediately opened his *Thorax* and *Abdomen*; and having fixed a glaſs Tube, which was $4 + \frac{1}{2}$ Feet high, to the deſcending *Aorta*, I then ſlit open his Guts from end to end in the ſame manner as in *Experiment* IX; then having poured blood warm Water on them, and covered them with a folded Cloth dipped in the ſame Water, warm Water was poured into the Tunnel, which when it had ſubſided to a Mark on the lower Part of the glaſs Tunnel, eighteen cubic Inches of warm Water were imme-

immediately poured in, out of a Pot which held juſt that Quantity: The Time that it was running thro' the fine capillary Arteries was meaſured by a Pendulum that beat Seconds.

4. I firſt poured in ſeven Pots full of warm Water, the firſt of which paſſed off in fifty two Seconds, and the remaining ſix, gradually in leſs time, to the laſt which paſſed in forty ſix Seconds.

5. Then I poured in five Pots of common Brandy, or unrectify'd Spirit of Malt, the firſt of which was 68″ in paſſing, the laſt 72″.

6. Then I poured in a Pot of warm Water which was 54″ in paſſing.

7. Hence we ſee that Brandy contracts the fine capillary Arteries of the Guts, and that Water ſoon relaxes them again, by diluting and carrying off the ſpirituous Part of Brandy, which as it is well known, not only contracts the Coats of the Blood-veſſels, but alſo thickens the Blood and Humours, both which Effects contribute to the ſudden Heating of the Blood, by much increaſing thereby its Friction in the contracted capillary Veſſels; which ſudden Heat is alſo further increaſed by the mere Mixture of Brandy with the Blood as

Dr.

Dr. *Boerhaave Elementa Chemiæ, Vol.* I. p. 366 observed, that on the mixing of cold Water and Spirit of Wine, they immediately acquired eight Degrees of Heat, so as to make the Mercury in *Fahrenheit's Thermometer* rise from forty four to fifty two Degrees; and sometimes in a like Mixture it has risen to fifty three Degrees, which Heat soon ceased, as does also that sudden glowing Heat which it gives the Blood. Hence it is that the unhappy habitual Drinkers of Brandy and other distilled spirituous Liquors, do so insatiably from time to time thirst to drink of that deadly Liquor, which by often heating the Blood and contracting the Blood-vessels, does by degrees reduce them to such a cold, relaxed and languid State, as most impetuously drives them to seek for their Relief in that Liquor, which they too well know both by their own Experience, as well as by the daily Destruction of Thousands, to be so very baneful and deadly, as to become by the great Abuse of them the most epidemical and destructive Plague that ever befel Mankind.

9. When cold pump Water which was fourteen Degrees above the freezing Point, was poured on the Guts, and some of it at

the ſame time poured thro' the Tunnel into the Arteries, thoſe capillary Arteries would on a ſudden be ſo contracted, as that the fourth Pot of cold Water was 80″ longer in paſſing thro' than the like Quantity of warm Water was juſt before: And the fifth Pot being warm Water, and the Guts at the ſame time being warmed by Water poured on them, it paſſed in 77″ ſooner than the former Pot of cold Water did.

10. Hence we plainly ſee how greatly Heat and Cold dilate and contract our Pores, which muſt therefore conſequently have a proportionable Effect on our Perſpiration, that great and important Evacuation: Thus hot Baths greatly increaſe Perſpiration, but a cold damp Air, and cold North-eaſterly Winds, by conſtringing the Pores retard Perſpiration, notwithſtanding there is no Abatement of the internal Heat: And on the other hand, when the Blood is cold, as in Dropſies, the Perſpiration will be much rebated for want of a due internal Heat, notwithſtanding the Pores may be relaxed: yet in a burning Fever, when by reaſon of its great Heat, the Pores ſhould be open, there is notwithſtanding very little Perſpiration; this as well as the other glandular Secretions being greatly

greatly diminiſhed and in a manner obſtructed, by the then viſcid State of the Blood.

11. When immediately after warm Water, three Pots of Water, ſo warm as that I could ſcarcely bear my Hand in it, were poured into the Arteries, the third Pot paſſed thro' in leſs time by 30″, than the preceding warm Water; and the next Pot of Water which was conſiderably hotter than the laſt paſſed off ſooner by 18″ than the foregoing. The like hot Water was at the ſame time poured on the Guts.

EXPERIMENT XVI.

1. I Made a ſtrong Decoction of Peruvian Bark, boiling a Pound of it in three Gallons of Water to two; when cold it was filtrated ſeveral times thro' a Flannel Bag. The Day following I prepared and ſlit the Guts of a young Spaniel Bitch as in the foregoing Experiment.

2. I firſt poured into the Tube which was fixed to the *Aorta* four Pots of warm Water, containing eighteen cubic Inches each, the laſt of which paſſed off in 62″ of time. Then I poured in ſucceſſively ſixteen Pots of equally warm Decoction of Bark, the firſt of which paſſed off in 72″ and the following ſlower

and slower, in proportion as the Vessels grew more and more contracted by the stiptic Quality of the Decoction, so that the 16th Pot was 224″ in passing.

3. Then I poured in successively eleven Pots of Water of the same Heat with the Decoction, the first of which Pots was 198″ in passing, and the succeeding ones passed sooner and sooner in proportion as they washed off the Decoction, and thereby relaxed the Coats of the capillary Vessels, till the eighth Pot which passed in 96″, after which the three following Pots of Water passed off in the same time, the Vessels relaxing no farther. It was not to be expected that the Water should so far relax them as to pass off in 62″ of time, which the abovementioned fourth Pot of Water did in this Experiment; for I always found that by a long Continuance to pour in Water, the Vessels did gradually let less and less pass; the capillary Vessels being compressed by the Water that insinuated into all Parts of the Coats of the Guts, so as to make them much thicker than they were at first; now the Constriction which is thus occasioned can only be inlarged by the Affusion of much hotter Water, as was done in the foregoing Experiment; whereas

whereas the Conſtriction of the Veſſels which is owing to the ſtiptic Quality of the Liquor, is we ſee, in this Experiment and others, manifeſtly taken away by the relaxing Quality of the Water, which waſhes away the ſtiptic Vertue.

4. I then poured in ſucceſſively five Pots of cold pump Water which was fourteen Degrees above the freezing Point, and whereas the preceding Pot of warm Water paſſed in 96″, the fifth Pot of this cold Water was 136″ in paſſing.

5. I tried in like manner a ſtrong Decoction of Oak Bark on another Dog, in which the preceding Pot of warm Water was 38″ in paſſing, but the following ſix Pots of Decoction ſo contracted the Veſſels that the laſt of them was 136″ in paſſing.

EXPERIMENT XVII.

1. HAving prepared a Decoction of twelve Ounces of Chamomel Flowers, boiled in three Gallons of Water to two, I poured it blood warm thro' the Arteries of the ſlit Guts of a large Pointer Spaniel. I found by the Velocity with which the four firſt Pots

of Decoction run off, that there was by Accident a large Branch of an Artery cut; which being stopped by a Ligature, I poured in successively eleven Pots of the Decoction, the first of which passed off in 96″ of time, the last in 138″; so that there is some Degree of Stipticity in this Decoction. Through inadvertency I omitted to pour in first some Pots of warm Water, by which the Degree of Stipticity would have been known more nearly.

2. Then the last of four Pots of scalding hot Water passed in 116″.

3. After these, six Pots of a Decoction of two Ounces of Cinnamon in a Gallon of Water being poured in hot, the Vessels gradually contracting, the last Pot was 216″ in passing. We see in this Instance how effectual Cinnamon by its great Stipticity is in stopping too large Discharges into the Cavity of the Guts.

4. Then a Pot of milk warm Water passed in 15″.

5. Next a Pot of scalding hot Decoction of Chamomel Flowers was 194″ in passing; which further shews its Stipticity.

EXPERIMENT XVIII.

1. HAving as in the three foregoing Experiments prepared and ſlit the Guts of a Dog, I firſt poured twelve of the abovementioned Pots full of warm Water thro' the Arteries, the firſt of which was 68″ in paſſing, the following Pots paſſed ſucceſſively faſter and faſter, to the four laſt which all paſſed off in 38″.

2. Then I poured in ſeventeen Pots of equally warm *Piermont* Water, the firſt of which was 40″ in paſſing, and the following Pots were in Succeſſion longer and longer in paſſing to the 17th which was 76″ Seconds in paſſing.

3. I then poured in ten Pots of equally warm Pump Water, which gradually relaxing the capillary Arteries again, each Pot paſſed off a little ſooner and ſooner, to the laſt, which was 64″ in paſſing off.

4. We ſee in this and the three foregoing Experiments, how the Veſſels of the Body are manifeſtly contracted or relaxed, by different Degrees of Warmth, Heat or Cold, or from the different Qualities of the Fluids which

paſs thro' them, as to Reſtringency or relaxing: And ſuch Qualities of the Fluids muſt have very conſiderable Effects on the finer capillary Veſſels, whoſe Coats bear a much greater Proportion to the contained ſmall cylindrical Fluid, than in larger Veſſels. Tho' it is not to be imagined that the Effects are ſo ſudden and great in a live Animal, as in theſe Experiments; becauſe in a live Animal, the ſeveral Fluids which are taken in, are more gradually and in ſmaller Proportion blended with the Blood.

5. 'Tis probable that ſuch things as conſtringe the Veſſels in any Degree, do alſo proportionably increaſe the Force of the arterial Blood, and thereby invigorate the Animal. For ſince the more the extream capillary Veſſels of the Arteries are contracted, ſo much the greater Force will be requiſite to impell equal Quantities of Blood thro' in equal times; the Blood muſt needs therefore be the more accumulated in the Arteries; and being thus impelled thro' ſmaller capillary Veſſels with greater Force, it will thereby both acquire a greater Degree of Heat, and be alſo the more attenuated and digeſted. 'Tis by this means that Bitters, ſuch as Chamomel Flowers and the Peruvian Park

may

may be greatly beneficial to the Blood, as well as by rectifying its *Craſis* in mixing with it as a Menſtruum. Thus the *Cortex* is in a double Capacity an Attenuator, both by reſtringing the Veſſels, and alſo by diluting as a Menſtruum the Fluid of the Blood, which it is well known to do, when mixed with extravaſated Blood: And in like manner Calybeats which are ſtiptic, do attenuate the Blood; thus alſo ropy Wines are cured by ſtiptic Attenuators, which precipitate the Tartar.

6. The ſudden glowing Warmth that Brandy gives, is owing not only to its raiſing a Warmth in mixing, as was before obſerved, with the Blood as a Menſtruum, as it does when mixed with cold Water; but to this alſo, that at the ſame time that it gives a contractive Spring to the Coats of the Veſſels, it alſo thickens the Blood, which muſt neceſſarily cauſe a much greater Reſiſtance and Friction between the contracted Veſſels and the more inſpiſſated Blood, which muſt needs therefore raiſe a greater Heat: Whence the Blood-veſſels in the Brain being dilated by theſe or other ſpirituous Liquors, and thereby too plentiful Secretions being made into the Subſtance of the Brain cauſe Intoxication and Sleep. Where-

as

as the *Cortex* by attenuating the Maſs of Blood, at the ſame time that it contracts the Veſſels, does not occaſion that ſudden glowing Heat that Brandy does, notwithſtanding it may perhaps equally contract the Veſſels. But when the Bark is given in the Paroxyſm of a Fever, it is obſerved to increaſe and prolong the Fever, which is probably owing to its reſtringing Quality, which by leſſening the Diameters of the Veſſels, in that too rapid State of the Blood, muſt neceſſarily increaſe its Heat.

7. It is by this Reſtringency alſo that the Bark is ſuppoſed to cure thoſe who are ſubject to profuſe Sweatings, it contracting the Pores.

8. The contractive Effects of different Liquors on the Veſſels may be of very different Length of Duration; thus that of Brandy is of ſhort Duration, for vinous Spirits are eaſily totally abſorbed by a more watry Fluid; whereas the Effects of other Reſtringents on the Solids, ſuch as *Cortex*, *Chamomel*, &c. *Piermont*, *Spaw* and other calybeate Waters are of a more permanent Duration. But thoſe who much accuſtom themſelves to drink ſtrong ſpirituous Liquors, do thereby deſtroy the Tone of the Fibres of their Veſſels, by having

them thus frequently, ſuddenly contracted, and ſo ſoon relaxed again; which make them like the Horſe Leech, be ever longing after and thirſting for more and more, thereby to regain the Tenſity of their too relaxed Fibres.

9. Thus what we take in, either as Food or Phyſick will have different Effects on our Solids as well as Fluids, according to their different Natures; and as a healthy State conſiſts in a due Equilibrium between the Solids and Fluids, it greatly imports, that they be rightly adapted to the different Conſtitutions, ſo as either to invigorate, contract or relax the Fibres of the Solids; or to change the Qualities or Quantities of the Fluids as occaſion ſhall require.

EXPERIMENT XIX.

1. THat I might know with what determinate Force I impelled Air into the Veſſels, I prepared the following Inſtrument, *viz.* I fixed to a common braſs cylindrical Air Force an Elder Stick, which was two Feet long, and two Inches Diameter; thro' which a Hole of half an Inch Diameter was made from end to end: One end of an inverted Glaſs Syphon

Syphon was laterally fixed into a Hole in the middle of the Elder Stick; then four Inches Depth of Mercury being poured into the Syphon, its other Orifice was cloſed up with Cement and a Piece of Bladder tied over it. When this Inſtrument was by means of a braſs Pipe fixed to any Veſſel of an Animal, I could by the Height of the Mercury in the mercurial Gage ſee with what Force Air was impelled.

2. When Air was with this Inſtrument forced into the deſcending *Aorta*, or the *Vena Porta*, none paſſed thence into the Cavity of the Guts, tho' Water by *Experiment* XIV. *Numb.* 12. did freely.

3. Neither when I ſlit a part of the Guts lengthways, would any Air paſs thro' thoſe tranſverſely cut, converging Arteries, when impelled with a Force equal to that of the arterial Blood: But when thoſe Arteries had been firſt waſhed, by the Flowing of warm Water thro' them, then the Air alſo would paſs freely; which ſhews how requiſite it is to waſh the Blood-veſſels, before Injections be made with coloured Liquors.

4. Tho' Air when blown in at the *Aorta* will not thence enter the Cavity of the Guts, yet

yet it will paſs thither when it lays in a latent State in the Interſtices of a Liquor: For having cut aſunder in two Places a Dog's Gut, at the Diſtance of two Feet Length, I waſhed that Piece of Gut clean, by paſſing of warm Water thro' it; then tying up each end of it, new frothy ſmall Beer which was blood warm, flowed from a perpendicular Height of 4+½ Feet thro' the deſcending *Aorta*, and thence after ſome time, in conſiderable Quantities into the Stomach and Guts. I found a Quantity of it equal to two cubic Inches in the Piece of Gut which was waſhed and tied up at each end; it was turbid and of a tawny Colour like the Grounds of Beer. When I warmed this again over the Fire, freſh Froth aroſe on the Top of it: An Argument that the flatulent Air which is generated in the Stomach and Bowels, may not ariſe wholly from flatulent Food, or ſome Irregularity in the Digeſtion of it; but that it may in part alſo ariſe from too flatulently diſpoſed Secretions that are made into the Stomach and Guts, from the Glands of thoſe *Viſcera*. If therefore there be at any time Air in the Blood-veſſels, it may, by being firſt reſorbed into the Blood, be by this means diſcharged from it in theſe plentiful Secretions.

EXPE-

Experiment XX.

1. WHEN Tubes were fixed at the ſame time to the left carotid Artery, and alſo to the left crural Artery, and Water poured into the Tube at the carotid Artery, ſo as to be four Feet ſeven Inches high in that Tube, then it would riſe four Feet four Inches in the other Tube: And when the Water ſubſided ſix Inches in the carotid Tube, it would proportionably ſink in the other Tube and riſe again when that was filled up. When the Column of Water in the carotid Tube was $9 + \frac{1}{2}$ Feet high, then it roſe in the other Tube eight Feet eleven Inches. The Inequality of the ſeveral Heights of the Columns of Water, was as I gueſs owing to the Smallneſs of the braſs Pipe which was fixed to the carotid Artery, on which Account it was not ſufficient to ſupply in full Plenty what ran off to waſte in the other Branches, and at the ſame time keep up the Water near to an equal Height in the other Tube, which it would doubtleſs have done, if by a larger Pipe the Tube had been fixed to the deſcending *Aorta.*

2. When

2. When I took away the Tube from the crural Artery, and the Water at the ſame time ſtood $9+\frac{1}{2}$ Feet high in the other Tube, then the Water ſpouted $11+\frac{1}{2}$ Inches out of the Artery which was held with its Orifice upright.

4. When a Column of Water was $4+\frac{1}{2}$ Feet high preſſed into the carotid Artery, then another Tube being fixed at the ſame time in the *Vena Porta*, pointing towards the Guts, the Water roſe ſlowly ſix Inches in the Tube: But when the Tube was fixed in the deſcending *Vena Cava* towards the Dogs Legs then no Water roſe in it.

4. If we could be ſo happy as to find a Liquor of ſuch a due Conſiſtence, as to paſs freely thro' from the Arteries to the Veins, as the Blood does in its natural State, then many curious and uſeful Experiments might in this manner be tried on ſeveral Parts of the Body.

5. It was with this View that I made Columns of warm Water flow into the Arteries of dying Dogs, at the ſame time that the Blood run out at the Veins, *viz.* to try if by this means I could waſh all the glutinous Blood out of the finer capillary Arteries and Veins, by making Water thus inſtantly follow

and

and impell the Blood out of them: But I was ſoon diſappointed in my Expectations; for the Water tho' a much more dilute Fluid than the Blood, had no free Paſſage from the Arteries to the Veins; the reaſon of which ſeems to be, that, as appears in many Inſtances in the ſecond and third Chapters of *Vegetable Staticks*, Water entering the Interſtices between the Sap-veſſels of the cut Branches, did in a few Days compreſs the Sap-veſſels, they withal at the ſame time contracting themſelves, ſo that no Water could enter them: And the Caſe is juſt the ſame here in Animals, for Water in many of the foregoing Experiments, freely paſſing from Arteries thro' innumerable Paſſages, which were too fine for the Blood Globules to enter, does thereby as in Vegetables, ſo compreſs the extream capillary Arteries, as to hinder a free Paſſage thro' them; and this is further confirmed by the Obſervation, *Numb.* 3. *Exper.* XVI. where it was obſerved, that the larger converging capillary Arteries of the Guts were gradually more and more compreſſed, by the Continuance of Water flowing thro' them.

6. We may hence alſo with good reaſon conclude, that the extream capillary Arteries,

are of a more elaſtic contractive Nature, ſo that the Water, not being like the Blood full of Globules, it cannot keep thoſe Veſſels open, as the Blood does by an uninterrupted Series of Globules following each other; and the greater Proportion the Quantity of theſe Globules bear to the more diluted part of the Blood, the leſs Quantity of Fluid will be ſecerned from the Arteries and *Vice Verſa.* And hence it is that it is found ſo difficult to inject with Vermilion or other coloured Liquors, the immediate Communications between the Arteries and Veins, in purſuit of which I made ſome few Attempts, *viz.*

EXPERIMENT XXI.

1. THO' I was ſenſible that anatomical Injections have of late Years been carried to a good Degree of Perfection by ſeveral skilful Anatomiſts; yet as they both prepared their Veſſels by injecting Water into them with a Syringe, and alſo injected their melted coloured Liquors by a Syringe; in uſing of which Inſtrument, one cannot be aſſured, with what Force either the Water or melted Liquor is impelled, ſo as to be ſecure from breaking the finer Veſſels, or from making Blotches by

too ſtrong an Injection; therefore, to remedy theſe Inconveniences, I thought it would be ſafeſt, as well as more effectual, to waſh the Blood-veſſels from a perpendicular Height of Water, which ſhould impell it with a Force no greater than that of the arterial Blood: And after the Veſſels were thus cleanſed, to make the Injections of coloured Liquors, by pouring them thro' hot Iron Tubes of ſuch Lengths as Experience ſhould prove to be beſt: This I hoped might be a likely Method to bring the Art of injecting to greater Certainty and Perfection. But tho' I have not ſucceeded herein ſo well as I expected, yet it may not be improper to give a ſhort Account of ſome few Attempts which I made in this Way; that more skilful Anatomiſts may judge whether they ſhall think it adviſeable to purſue the Matter further; in which Purſuit I am perſuaded they would not loſe their Labour.

2. When I intended to inject coloured Liquors into the Blood-veſſels of a Dog, having fixed a Tube $4+\frac{1}{2}$ Feet high to his left carotid Artery, and then cut both jugular Veins open, I immediately poured blood-warm Water down the Tube, to waſh the Blood out of the

the Veſſels: Then as ſoon as the Dog was dead, I opened his *Abdomen*, if I deſigned to inject the lower Parts, and cut open the *vena Porta*, and deſcending *Cava*, to give room for the Water to waſh the Blood from the correſponding Arteries into them; for the Blood having no Paſſage in the dead Dog, thro' the Liver, it was ſo pent up in the *Porta*, as to ruſh out forcibly when that Vein was cut. Then the Water continued to flow thro' the Arteries from the Tube; which was ſupplied either from a large Veſſel of warm Water whence it run, or elſe by lading it into the Tunnel; this was continued for half an Hour, and ſometimes an Hour or more, till the Guts and Stomach were white.

3. Then opening the *Thorax*, I fixed a braſs Pipe to the deſcending *Aorta*, and to the Pipe was fixed to a Gun Barrel, which was heated by pouring boiling Water into it, ſeveral times, and with which it ſtood filled till wanted, its great End ſtanding alſo ſome Depth in a Veſſel full of hot Water: Then the injecting Liquor being duly heated, was poured down the Barrel till it was full, thro' an Iron Tunnel which was fitted to the Outſide of the Barrel, that it might have a larger Orifice for Liquor to flow in faſt.

4. I had at firſt no braſs Cock at the Bottom of the Barrel, but being apprehenſive that the Liquor which firſt flowed into the Arteries, before the Barrel was full, might not go to the Extremities, for want of a ſufficient Impetus from a greater perpendicular Height, I therefore fixed a braſs Cock at the bottom of the Barrel, which being ſtopped till the Barrel and Tunnel was full, was then immediately opened to let the Liquor ruſh into the Arteries with a full *Impetus*. This Method did ſometimes ſucceed pretty well, but ſometimes no better than the other, which made me ſuſpect, that the Vermilion might by its great ſpecific Gravity, ſubſide in too great Quantity towards the Bottom of the Barrel, before the Cock was opened.

5. One of the Barrels was $4+\frac{1}{2}$ Feet, and the other $5+\frac{1}{2}$ Feet long; I ſometimes uſed them ſingly, and ſometimes joined together in one Tube; this Column of ten Feet never cauſed any Blotches in the Injection.

6. The Mixture I made uſe of was the following, *viz.* white Roſin and Tallow of each two Ounces, melted and ſtrained thro' Linnen, to which was added three Ounces of Vermilion, or finely ground Indigo, which was firſt well

well mixed with eight Ounces of Turpentine Varniſh. I was obliged to Mr. *Ranby* for communicating to me this Receipt.

7. I kept the Bowels warm, either by frequently pouring warm Water on them, and covering them with a wet Cloth, on which warm Water was often poured, or elſe by immerſing the whole Dog in a Veſſel of warm Water. I believe I ſhould have ſucceeded better if this Water had been as hot as that mentioned under *Experiment* XV. *Numb.* 11. for that Degree of Heat, as we there ſee, not only diſpoſes the fine Veſſels to dilate much, but coming alſo near to the Heat of the melted injected Liquor, it will not cool ſo ſoon, whereby it will have more time to inſinuate itſelf into the finer Veſſels; which Inſinuation is greatly promoted by the conſtant even Preſſure of the Column in the Barrels; for which Reaſon I did not take away the Barrels till all was cold.

8. I was in hopes by this means to have fixed the coloured injected Liquor, in the immediate Communications between the Arteries and Veins, but could not ſucceed ſo well as I expected to have done, notwithſtanding the Injection paſſed from the Arteries into

the Veins of the Stomach, Guts and Urine-Bladder, but eſpecially in the Gall-Bladder; and carried with it, ſometimes a little Vermilion and ſometimes none. I have a Gall-Bladder which was injected from a Column $4+\frac{1}{2}$ Feet high, without a braſs Cock at the Bottom of the Barrel, all the Blood-veſſels of this Dog had firſt been waſhed for an hundred Minutes, from a Column of Water $9+\frac{1}{2}$ Feet high: The Arteries and Veins of this Gall-Bladder are both injected, and there is a conſiderable Quantity of Vermilion in the Veins, tho' not ſo much as in the Arteries; I can plainly ſee with a Microſcope here and there an extream Artery injected to the very Coat of the Vein, which it enters at right Angles; ſo that the Blood circulates by immediate *Anaſtomoſis* of the Arteries and Veins, without the Interpoſition of any glandular Cavities.

9. The immediate Communication between the Arteries and Veins ſeems to be in this manner, *viz.* the Arteries which converge and inoſculate into each other, ſend out of each of their converging Sides Branches at right Angles to thoſe Sides, which Branches ſoon divide, like the ſpread Fingers on the Hand, into ſeveral much finer Branches; and theſe into others,

ſome fewer, ſome more, according to what part of the *Areola* or Net-like Meſh they are to reach to; hence they dip at once, at right Angles down to the Veins, which they enter alſo at right Angles, ſome into the larger converging Veins, others into the leſſer Veins, which like the Arteries branch at right Angles from the converging Veins, and ſpread like them the *Areolas* or Meſhes: But the *Areolas* between the converging Arteries are much larger, and more nearly right ſided and rectangular than the *Areolas* of the converging Veins, which approach nearer to Circles.

10. The great Diſproportion there is, between the Force of the arterial and venal Blood, ſhews how needful it was, not only to have all the Communications between the Arteries and Veins ſo very ſmall that only ſingle Globules can paſs; but alſo to have the Blood both paſs from the Arteries at right Angles, and alſo to enter the Veins in the ſame manner, which muſt greatly contribute to the retarding of its Motion, eſpecially in very fine Channels. Elſe the arterial Blood would ruſh with ſuch Rapidity, as thereby to make the Force of the Blood in the Arteries and Veins come nearer to an Equilibrium; the obvious Conſe-

quence of which would be, that the Blood could not be impelled thro' the finer capillary Veſſels, beſides many other Inconveniences that might be enumerated. But the Extremities of the Arteries are not in all Parts alike, for in ſome Places they do not form a Network; ſo that the Paſſage of the Blood from the Arteries may be in a very different manner in different Places.

11. Since from *Experiments* XV and XVI, *&c.* it is found that where Water has long paſſed thro' the Arteries, it is apt to contract the Veſſels by dilating the adjoining Parts; and ſince in *Experiment* XXIII, *Numb.* 7. it is obſerved that when the Coats of the Stomach have been thus filled with Water, on blowing up the Stomach that Water has flowed out plentifully from thoſe Coats of the Stomach; it ſhould therefore ſeem a good Method, to diſtend the Stomach and Guts with Wind for ſome time, in hopes to get out a good Part of the Water which was lodged in their Coats, while the Blood-veſſels were waſhing clean, taking care to immerſe the Dog all the while in warm Water. 'Tis probable that by this Means the Blood-veſſels would be well cleanſed and emptied, and thereby rightly prepared

to

to make a good Injection. And if one of the crural Arteries were opened and had a Ligature put round it, which Ligature might be inſtantly tied as ſoon as the injected Liquor had forced the Water and Air out of the *Aorta*, which muſt elſe have been drove before the injected Liquor into the finer capillary Veſſels, which doubtleſs often hinders the Injection from penetrating ſo far as it would otherwiſe do.

12. Some of the Vermilion came always into the Cavity of the Bowels, tho' the Injections were made with no greater Force than that of the arterial Blood, *viz.* with a Column of $4+\frac{1}{2}$ Feet Height; and it was the ſame whether the Injection were made by the *Aorta* or *Vena Porta*, for in both caſes, the red Streaks of Vermilion might be ſeen with a Microſcope in the *Papillæ* of the mucoſe Coat of the Bowels.

13. As none of this Vermilion paſſed into the Lymphaticks, fat Veſicles, or extravaſated Parts, as the Water did, this is a Proof that the Water which was impelled with no greater Force than the Vermilion, did not burſt any Veſſels, when it became extravaſated, but that it paſſed thro' the fineſt ſecerning Tubes, as alſo thro' the Pores of the Veſſels, where viſcid Blood cannot paſs.

14. I waſhed the Blood-veſſels of a Dog with ten Quarts of Water, in which five Ounces of Nitre had been diſſolved, to try whether this Liquor would more effectually cleanſe the Veſſels, but it had the contrary Effect; for all the Parts of the Body continued very red, after long waſhing, ſo that the Nitre which attracted ſtrongly the more ſulphureous red Globules, fixed them in all the Veſſels. It was very remarkable that none of this Dog's Muſcles were convulſed by this nitrated Water, as they conſtantly were, when fair Water paſſed into the Arteries. All the Blood of this Dog was very florid as it flowed out at the jugular Veins.

15. When a Gallon of Water was injected into a Dog's Arteries, in which Water two Ounces of Sal Armoniac was diſſolved, all Parts were alſo very red. But I neglected to obſerve whether his Muſcles were convulſed or no.

EXPERIMENT XXII.

1. AFter having ſeen many Proofs of the great Force of the Blood in the Arteries, and alſo in the Veins when an Animal exerts

exerts its Strength; I thought it might not be a useless Enquiry to examine the Strength of the Coats of those Vessels.

2. I poured into an inverted glass Syphon some Mercury, so as to have the shortest Leg, which was hermetically sealed, filled within four Inches of the Top: To the other open end of the Syphon I fixed by means of a brass Pipe, one end of the right carotid Artery of a small Spaniel Dog, and to the other end of the Artery was fixed a condensing Syringe. Then placing the Artery in Water, to see whether it leaked, I impelled Air in to such a Degree, as made the Mercury compress the Air between it and the sealed Top of the Tube into so small a Compass, as shewed by Estimation, the Force to be equal to a Column of Water a hundred and ninety Feet high, or equal to the Weight of 5.42 Atmospheres; with this Force the Artery burst at once, but no Air passed thro' its Coats before it burst.

3. The Diameter of this Artery being 0.1 Inch, the Circumference is 0.314, the whole Surface of an Inch Length of the Artery will be 0.314 of a square Inch: Now a Column of Water whose Base is a square Inch, and its Height a hundred and ninety Feet weighing

81.9 Pounds, 0.314 part of that is 25.71 Pounds, equal to the Force which an Inch Length of the Artery ſuſtained, when it burſt. And its Diameter being 0.1 Inch, the one tenth part of 81.9 Pounds or 8.19 Pounds is the Force which was requiſite to burſt aſunder the Fibres in an Inch Length of the greateſt Section of that Artery. The Force of the arterial Blood in a Dog, impelling it but eighty Inches high in a Tube; this is but $\frac{1}{26.7}$th Part of the utmoſt Strength of the Artery, an Allowance being here made for the Difference between the ſpecific Gravity of Blood and Water.

4. When the Velocities of the Pulſes are increaſed by the briſk Motion of the Body, as in a Man from ſeventy five to a hundred and twenty, ſo in a Dog from ninety ſeven to a hundred and forty two in a Minute, the Quantities thrown out of the left Ventricle of the Heart, cannot be proportionably increaſed; for the left Ventricle cannot receive and throw out as much Blood in each very accelerated Pulſe, as in the natural ſlower ones: Beſides as the Force of the Blood increaſes in the Arteries, ſo will it on that Account paſs off the faſter, as alſo on account of the

extream capillary Veſſels being inlarged, both by the greater Force and Heat of the Blood. It may therefore be reaſonably concluded, that in the greateſt Motion of the Body, and quickeſt and fulleſt Pulſe, the Force of the Blood will not riſe proportionably from eighty to one hundred and ſeventeen Inches; but may reaſonably be ſuppoſed not to exceed a hundred Inches; that is but $\frac{1}{21.5}$th Part of the Force which burſt this Artery.

5. The carotid Artery of the Mare uſed in *Experiment* III. was ſo ſtrong that I could not break it with my Condenſer.

6. Her jugular Vein burſt with a Force equal to a Column of ſeventy ſix Feet Height of Water; but it was in a part where by often bleeding it was near double its natural Diameter. The other Part of that Vein which was $\frac{1}{2}$ Inch Diameter, burſt with a Force equal to a Column of Water one hundred and forty four Feet high. A jugular Vein of another Horſe bore a Preſſure equal to a hundred and forty eight Feet Height of Water and did not burſt, but had ſome ſmall Leakages, out of which a little Air eſcaping prevented its total burſting.

7. This

7. This Vein being 0.5 Inch Diameter, an Inch Length of its whole Surface will be 1.57 ſquare Inch; and a Column of Water whoſe Baſe is a ſquare Inch, and its Height a hundred and forty eight Feet will weigh 62.9 Pounds, which multiplied into the Area of an Inch Length of the Vein gives 97.75 Pounds, the Weight which that Inch Length of Vein ſuſtained without burſting. And the Area of its greateſt Section Lengthwiſe being 0.5 ſquare Inch, the Fibres in that Section ſuſtained 31.45 Pounds without breaking.

8. Now ſuppoſe the ordinary Force of the Blood in the jugular Vein of a Horſe to be equal to twelve Inches Height of Blood, this will be but $\frac{1}{137.2}$ of the Force which it ſuſtained without burſting. And ſince when the Horſe ſtrained to get looſe the Blood roſe 52 Inches high in the Tube fixed to his jugular Vein, and would have riſen ſomewhat higher if the Tube had been longer, ſuppoſe to ſixty Inches, this will be but $\frac{1}{27.4}$th Part of what the Vein was able to ſuſtain.

9. I impelled Air into a Piece of the right jugular Vein of a Dog, to ſuch a Degree as to compreſs the Air in the mercurial Gage, with a Force equal to the Weight of five Atmo-

ſpheres,

ſpheres, or to a Column of Water five times thirty five Feet high, that is, a Column a hundred and ſeventy five Feet high, yet this Force did not break the Vein, but one of the Ligatures failed.

10. The Diameter of the Vein being 0.25, the Surface of an Inch Length of it will be 0.785 ſquare Inch, this multiplied by 76.1 Pounds the Weight of a Column of Water whoſe Baſe is a ſquare Inch, and its Height a hundred and ſeventy five Feet, gives 59.7 Pounds, the Weight which preſſed on an Inch Length of this Vein; whoſe Diameter being 0.25 the Area of its greateſt longitudinal Section is 0.25, ſquare Inch, which multiplied into 59.7, gives 14.9 Pounds, the Weight which the Fibres in that Section ſuſtained.

11. The common equable Force of the Blood in the jugular Veins of a Dog, being about five Inches, this is but $\frac{1}{399}$th Part of the Force which this Vein ſuſtained without burſting. And if we ſet the Force of the Blood in ſtraining, to be twenty four, ſufficient to raiſe it twenty four Inches in a Tube, which was the Height to which it roſe, when the old Dog, *Numb.* 10. ſtrained, ſee the Table, *p.* 42. then this Force will be $\frac{1}{83}$d Part

of

of the Force which this Vein ſuſtained: An Allowance in both theſe Caſes is made for the different ſpecific Gravity of the Blood and Water. As there is doubtleſs a very great Difference in the Strength of the Fibres of young and old Animals, I had intended to have enquired in the ſame manner, into the comparative Strengths, not only of their Arteries and Veins, but alſo of their Guts. And if the Strength of the Perioſteum and Ligaments of the Joynts were tried in the ſame manner, as in *Numb.* 29. of this *Experiment*, both on old and young Animals, there would be found a conſiderable Difference.

12. We ſee in theſe Inſtances the great Strength of the Coats of theſe Veſſels; what great Reaſon have we therefore with thankful Hearts to ſay to our Creator, as holy *Job* did when he contemplated on the wonderful Frame and Strength of his Body, *Job.* x. 11. *Thou haſt* not only *fenced me with Bones and Sinews*, but haſt alſo effectually ſecured the vital Fluid, in ſuch ſtrongly wrought Channels, as are Proof againſt its moſt lively and vigorous Sallies, when either agitated by the different Paſſions, or by ſtrong or brisk Actions of the Body.

13. It was computed above, that the Force of the Blood in the Dog's Artery could not probably increaſe more in its moſt violent Motion than from eighty its natural State to a hundred; that is one fifth part more. But in the Veins the Difference is manifeſtly greater; for in the Mare it roſe from twelve to fifty two Inches, and would probably have riſen to ſixty Inches if the Tube had been longer, *i. e.* five times its natural equable Height. And in the Dog, *Numb.* 10. it roſe from five to twenty four Inches, that is, near five times as high:

14. Now it was obſerved in *Experiment* VII. *Numb.* 4. that when a Dog's Belly was compreſſed with the Hand, the Blood would conſtantly riſe a few Inches in the Tube fixed to his Artery, and ſubſide again when the Hand was taken off; and in like manner the increaſed Force of the Blood in the Veins, ſeems to ariſe chiefly from the Conſtriction of the *Abdomen*: For when we exert our Strength in lifting or the like, we conſtantly contract the *Abdomen* what we are able, by the joint Action of all its ſurrounding Muſcles; and the Diaphragm is alſo further aſſiſted in that Action, by keeping the Lungs and Thorax diſtended with Wind, which is there pent in by

hindering its Expiration at the Mouth and Nose: And as we cannot thus hold our Breath long, so neither can we thus exert our utmost Strength long, without some short Intervals of Abatement. And during this straining we constantly see the Veins of our Neck, Forehead and Temples much distended; the Blood being forcibly impelled into them from the much contracted *Abdomen*, in whose large Veins there is Plenty of it in store; for the Number and Capacity of all the Veins of the Body is observed to be much greater than that of the Arteries.

15. And while the Blood is thus strongly compressed in the Veins, it must proportionably retard its Passage out of the Arteries, it being therefore there accumulated, will acquire an additional Force of four Feet perpendicular Height, that is in all equal to $13 + \frac{1}{2}$ Feet in the straining Mare: And an additional Force of twenty four Inches, equal in all to eight Feet and eight Inches in the straining Dog; whereby the Blood being impelled more forcibly into the Muscles enables them to act the more vigorously.

16. But the free natural Course of the arterial Blood into the Veins is not only retarded by

by the great Compreſſure of the *Abdomen* on its Veins, but alſo when thoſe Veins (which are the great Store-rooms of both the neceſſary and too redundant Fluids) are too plethoric, in proportion to the Degrees of which, the free Courſe of the arterial Blood will be rebated: And the Heart being in this caſe like a Water-Mill that is ſurcharged with a Flood both before and behind, its Force muſt needs abate and grow languid; in which caſe Evacuation by bleeding, is well known to reſtore it to its natural Vigour.

17. When the Courſe of the Blood is retarded by ſome Defect in one part, it occaſions thereby a greater Flow of it on other Parts: Thus ſome of thoſe that have a Limb cut off, have been obſerved to be ſubject to Hæmorrhages; and for the ſame Reaſon a ſcirrhous Liver and Spleen will cauſe a vomiting of Blood, hence alſo when the Spleen is cut out, the Liver increaſes.

18. As the Vigour of the Blood in the Arteries and Veins is thus greatly increaſed by the increaſed Preſſure on the Blood-veſſels in the *Abdomen*; ſo is its Vigour much rebated when on the contrary the Blood-veſſels of the *Abdomen* are too little compreſſed; thus when in

 a Dropſy

a Dropſy a great deal of Water is at once drawn off by Tapping, the Patient is then in danger of dying in the Operation ; for which reaſon, when the Quantity of Water is large, it is drawn off not at once but at ſeveral times, that in the Intervals the dilated Parts of the *Abdomen* may have time to contract, and thereby duly compreſs the Blood-veſſels ; which are there made large and capacious, that they may be fit to receive the greater Stores, with which they are repleniſhed ſometime after Meals.

19. And thus alſo the ſmall Evacuation of a Clyſter will ſometimes cauſe a ſick Perſon to faint, or to be ready to faint ; an Argument that the vital Force of the Blood does then run low.

20. Thus alſo a natural Looſeneſs or a Purge do very ſenſibly rebate the Vigour of the Blood, by too much evacuating the *Abdomen*, as well as by the extraordinary Quantity of Fluid, that is in thoſe Caſes diſcharged from the Blood into the inteſtinal Tube. In which greatly rebated State of the Blood, the Surface of the Body being thereby reduced from a vigorouſly perſpiring, even ſometimes to an imbibing State, we are thereby the more ſubject to catch cold.

21. When

21. When the Veſſels are in ſome Degree emptied by bleeding, there being then a leſs Quantity of Blood in the Arteries and Veins, and conſequently a leſs Quantity paſſing in equal times thro' the Heart, it finds proportionably leſs Reſiſtance from the arterial Blood, whence an Abatement in the Vigour of the Pulſe. It being alſo thereby leſs forcibly propelled thro' the capillary Veſſels, and conſequently undergoing there much leſs Friction it grows ſenſibly cooler.

22. When a Vein is opened the Blood flows for that Time, not only the faſter in that Vein, but alſo in its correſponding Arteries; whence it is found by Experience that bleeding near the Part affected is in ſome caſes moſt beneficial; becauſe the fine capillary Veſſels of that part, in which the regular Courſe of the Blood is retarded, will by this means be ſooner drained, than if the Bleeding were at a more remote Part; eſpecially at the beginning of the Diſeaſe, before the Obſtruction be too firmly fixed, when bleeding near the affected Part may rather increaſe than abate the Inflammation. As bleeding or not bleeding in due time and Proportion are frequently of the utmoſt Conſequence to the Patient, ſo it requires the

Judgment of the skilful Phyſician to know when and in what Proportion to abate by this Evacuation the Force of the Blood. And in general ſince a human Body is ſo curiouſly complicated an Engine, that the mutual Harmony of ſuch innumerable Circumſtances muſt concur to its Welfare, no wonder that it requires the maſterly Hand of the skilful Phyſician to rectify it when out of order. Were the bold Empiric but duly ſenſible of this, he would not ſurely dare to intermeddle, but Ignorance is his beſt Plea.

23. Tho' the Strength of the Coats of the finer Arteries and Veins are proportionably thinner and weaker as they grow leſs and leſs; yet ſince the Sum of the Preſſure of equally high Columns of Water againſt the Sides of containing cylindrical Tubes of different Diameters is proportionable to the Surfaces of thoſe Tubes; the Strength therefore of their Coats may be equally as much ſuperior to the utmoſt Efforts of the Blood in them, as thoſe of the larger ones are to the Force of their contained Blood; they are therefore proportional to their Circumferences.

24. But as to the finer ſecretory Veſſels and Lymphatics which ariſe from the capillary Arteries,

Arteries, and are out of the Courſe of the circulating Blood : Theſe as they do not ſuſtain the full *Impetus* of the arterial Blood, but only ſerve more ſlowly to ſecern and convey the more diluted Part of the Blood, which is proper either for Nutrition, Perſpiration, *&c.* ſo neither are their Coats near ſo ſtrong as thoſe of the Arteries and Veins; as is manifeſt from *Experiment* XIV. *Numb.* 6. where when Water flowed into the Arteries, with a Force no greater than that of the arterial Blood ; yet it paſſing freely by reaſon of its great Diluteneſs, into thoſe ſecretory Veſſels did thereby much dilate all the Parts of the Body. And it is obſerved that as the elaſtic Power of thoſe Veſſels, and alſo the Force of the Liquor that flows in them is but ſmall, ſo are Obſtructions apteſt to be in the Glands, where theſe Veſſels are both very complicated and numerous.

25. Tho' in a living Animal there is very rarely if ever, ſuch an univerſal Inundation and Dilatation of all the Parts, as is here made by theſe Experiments on Dogs ; yet it frequently happens, that on the Stoppage of one of the natural Evacuations thro' theſe excretory Ducts, that others will be increaſed and *vice verſa.*

Thus when there is too plentiful a Diſcharge into the Cavity of the Guts in a great Diarrhœa, the Perſpiration is then ſmall, and there is little *Saliva*. On the contrary in the Quinzey the *Saliva* abounds, becauſe the Courſe of the Blood is there obſtructed: As alſo in the ſmall Pox the Quantity of *Saliva* is large, becauſe the Perſpiration is ſtopped; which Stoppage of Perſpiration often alſo increaſes the Quantity of the *Mucus* ſeparated in the Noſe. And from the Stoppage of Perſpiration, rheumatic Pains are cauſed by too great Diſcharges of theſe Secretions among the Fibres of the Muſcles, in the ſame manner as was manifeſtly to be ſeen in the Muſcles of theſe Dogs.

26. It has been obſerved, on drinking a great Quantity of Water as three or four Pints at a Time, that all Parts of the Body and even the Fingers have been dilated: In this caſe doubtleſs part of this great Quantity of Water, being not throughly incorporated with the Blood, paſſes thus freely into the ſecretory Arteries and Lymphatics: The drinking of Waters ſhould ſeem therefore to be of uſe in many kinds of Obſtructions in theſe Veſſels.

27. Since the Coats of theſe Veſſels are found to be ſo much weaker than thoſe of the

Arteries, from which they branch off, and ſince the capillary Arteries are ſtrong enough to ſuſtain the Impetus of the Blood in violent Exerciſes without ſwelling; it may therefore reaſonably be concluded, that Inflammatory and other Swellings are often owing to the Serum's or Lymph's being, on a Stoppage in the capillary Arteries, impelled with greater Force into theſe ſecretory Veſſels, which eaſily dilating at each Pulſe, occaſions the Throbbings of ſuch Swellings: Which Swellings, compreſſing the Blood-veſſels as was obſerved in *Exper.* XVI. *Numb.* 3. the Blood muſt therefore neceſſarily paſs the adjoining capillary Arteries with greater Difficulty, whence there muſt needs be a greater Friction and Heat; and this inflammatory Heat may ariſe to a great Degree, if the mucoſe Coat of the Veſſels is thereby rubbed off, when the Globules themſelves may alſo be ſtripped of their oily Covering. Hence alſo on catching cold theſe ſecretory Veſſels being too full for want of a due Perſpiration, may by compreſſing the capillary Arteries, cauſe a feveriſh Heat, as well as by diſordering the *Craſis* of the Blood. Anatomiſts have found on tying the jugular Veins of a live Dog, and thereby

greatly

greatly increasing the Force of the Blood in the Head, that the Head has swell'd: As also by tying the *Vena Cava* the *Abdomen* was filled with Water. Thus also when the Texture of the Blood Globules is dissolved to such a Degree as easily to enter these finer secretory Ducts, then malignant Inflammations are produced.

28. To these Experiments on the Strength of the Coats of the Blood-vessels, it may not be improper here to subjoin some that I made ten Years since to shew the Strength of the *Periosteum* and *Ligaments* of the Joints, *viz.*

29. I took the Instep Bone of the hinder Leg of a Calf, which was nine Weeks old, which Bone reaches from the Hock down to the pastern Joint, to which it is joined by a Joining called *Ginglimus*. I cut off from the Bone all the *Tendons*, *Ligaments* and *Periosteum*, and bored a Hole with a Gimblet, thro' the Head of the lower *Ginglimus* End of the Bone; thro' which Hole I thrust an Iron Pin, which hindred the Noose of a Cord that was fastened to it, from slipping off the Head of the Bone. I fastened the other end of the Bone with a strong Cord to the Threshold of a Door Case: and then put the End

of

of a long Iron Bar thro' a Nooſe of the Rope which was faſtenened to the *Ginglimus* End of the Bone. The Bar reſted on a *Fulcrum*, ſo as to become a Lever: Then I hung on the other end of the Lever Weights ſufficient to draw off the Head of the Bone at the joining where it grows in length, which is called *Symphyſis*; I found the Tenacity and Reſiſtance of this joining, to the Weight, to be equal to one hundred and nineteen Pounds when the *Perioſteum* was taken off.

30. Then I took the Inſtep Bone of the other Leg of the ſame Calf, and cut off only the Tendons, but left the *Perioſteum* entire, and then fixed it in the ſame manner as the former to the Lever; whereby I found the Force requiſite to ſever the Head of the Bone from its Shank to be equal to five hundred and fifty Pounds.

31. By the Experiment on the firſt of theſe Bones, the Tenacity of the viſcous Matter, at the joining of the Head to the Shank, was equal to one hundred and nineteen Pounds, which deducted from five hundred and fifty Pounds in the ſecond Experiment, there remains four hundred and thirty one Pounds, which gives the Strength of the *Perioſteum*; which

which is therefore doubtleſs, among other uſes, of great Importance in ſtrengthening the Bones to which it moſt cloſely adheres. The Circumference of the Bone where the joining *Symphyſis* was forcibly ſeparated, was about four Inches, ſo that the Strength of one ſquare Inch of the *Perioſteum* is equal to the Force of one hundred Pounds Weight, which Strength is much greater than the above found Strength of the Coat of the Arteries and Veins; the great Author of Nature ever proportioning the Strength of all the Parts of the Body to the ſeveral Offices they are to perform.

32. I forcibly pulled aſunder in the ſame manner the Hock Joint of one of thoſe Legs, having firſt taken off the Muſcles and Tendons. Whereby I found the Strength of the Ligaments with which that Joint was braced, to be equal to the Force of eight hundred and thirty Pounds Weight: So great Care has Nature taken to prevent the Luxation of the Joints, and ſo ſtrongly are we *fenced with Bones and Sinews*, Job x. 11.

33. As a Force equal to five hundred and fifty Pounds was found requiſite to ſeparate the abovementioned joining *Symphyſis*, ſo in the Growth of the Bone lengthwiſe at that joining

ing Nature muſt exert a like Power; not that we are to ſuppoſe that the growing Fibres are forcibly ſtretched out at each End, in the manner they were in this Experiment: Nature in the extending a growing Fibre rather effects it by the gentle Expanſions of Warmth, which exerting its Efficacy on every individual Part of each Fibre, does thereby gradually lengthen it. But yet the whole Sum of this Power muſt be ſuperior to the Reſiſtance of all the Fibres which connect this Juncture.

34. It may not be improper here to ſubjoin an Account of ſome of the like Experiments which the ingenious and learned Profeſſor *Peter Van Muſſchenbroek* of *Utrecht* made on ſeveral other animal Subſtances, *viz.* " ſingle " Threads of Silk from the *Chryſalis* of a " Silk Worm were torn aſunder, ſome with " the Weight of eighty Grains, and others " with intermediate Weights from eighty to " ninety Grains. *Introductio ad cohærentiam* " *corporum firmorum*, p. 520, *&c.*

" 35. Fifty ſeven of thoſe Threads, when " but little twiſted were as big as a human " Hair, theſe ſuſtained 4845 Grains, when " they break aſunder, *viz.* each eighty five " Grains.

" 36. Affair

" 36. A Hair of a healthy young Man's " Head, just then pulled off, sustained 2069 " Grains.

" 37. Seven of these Hairs when twisted " were equal in Size to a Horse's Hair, which " being untwisted sustained 9635 Grains.

" 38. A Horse's Hair sustained 7970 or " 7920 Grains. So this Hair which was 399 " times bigger than a single Silk Thread sus- " tained but 7970 Grains, whereas 399 Silk " Threads would sustain 33915, that is 4.2 " times more.

" 38. A Spider's Thread, sixteen of which " was equal to a human Hair, sustained a " hundred and fifty Grains, and sixteen of " them 2400 Grains.

" 40. Twenty three Threads of the Fi- " bres of Flax, which all together were equal " to the Hair of a Horse, sustained 11710 " Grains; each of these Threads, when view- " ed with a Microscope, might be seen to be " composed of at least fourteen more.

" 41. Now supposing all these abovementi- " oned Threads to be of the Size of a Horse's " Hair; then the Number of Grains which " each can sustain will be as follows, *viz.*

"	Silk	Spider's Thread	Flax	Human Hair	Horſe Hair
"	33915.	15800.	11710.	9635.	7970.

" Whence he collects, that the finer the com-
" ponent Threads are, the ſtronger they are.

" 42. He found alſo that a Cord of a mu-
" ſical Inſtrument which is made of Animals
" Guts, ſuſtained thrice as much as a Horſe
" Hair of equal Size.

" 43. A Thong of Leather of a Calf's
" Skin which was 0.4 twelfths of an Inch
" broad, and 0.08 thick, ſcarce ſuſtained eigh-
" ty Pounds.

" 44. A Thong of Leather of an Ox's
" Hide which was 0.4 twelfths of an Inch
" broad, and 0.18 thick, ſuſtained three hun-
" dred and eighty Pounds, whence he takes
" occaſion to eſtimate the Strength of the
" leathern Braces by which a Coach is ſuſpend-
" ed, *viz.* for ſince ſuch a Thong of four
" Lines or ⅓d of an Inch Breadth will ſuſtain
" three hundred and eighty Pounds, one of
" three Inches Breadth will ſuſtain 3420
" Pounds, and five ſuch ſewed together to
" make a Brace may ſuſtain a Weight equal
" to 17100 Pounds; and four ſuch Braces
" doubled,

" doubled, as they are in Coaches, will bear
" 68400 Pounds, *Amſterdam* Weight, which
" being to *Engliſh* Avoir-dupois, as ninety three
" to a hundred, will be 63611 Pounds *Engliſh*. The Inch alſo here uſed by him is
" the *Rynland* Inch, which is to the *Engliſh* Inch as 0.752 to one.

" 55. He plainly proves alſo by many Experiments, that a twiſted Rope cannot ſuſtain near ſo great a Weight as all its ſingle untwiſted Threads can do. And that a Rope which is breaded like Womens Hair, is ſtronger than one twiſted in the common Way, in the Proportion of one hundred and ſeventy to twenty: Diſcoveries which may be of great Uſe in many Inſtances.

56. It is well known that a twiſted Rope or Cord does contract or ſhorten on being wetted; the imbib'd Moiſture dilating laterally their twiſted Threads, whence the Cord muſt contract longitudinally; but on the contrary ſingle untwiſted hempen or flaxen Fibres, relax on being wetted, as animal Fibres do, tho' not ſo much.

EXPERI-

EXPERIMENT XXIII.

1. I Made hydraulic and hydroſtatical Experiments not only on the Arteries and Veins, but alſo on the inteſtinal Tube; by affixing in like manner Tubes of different Heights to each end of them, while they were warm.

2. I fixed a Tube to the Gullet of a Dog, and then poured in Water till the Stomach was ſo full, that the Water in the Tube ſtood thirty ſix Inches perpendicular Height above the Stomach; which Force burſt it lengthways in its upper part near the *Pilorus*, where it was but $7+\frac{1}{2}$ Inches in Circumference: Yet no Water was impelled thro' the *Pilorus* with this Force, tho' in ſome other like Experiments it has run thro' there into the Guts. Another Dog's Stomach burſt in the larger left part of it, when the Height of the Column of Water in the Tube was but thirty Inches.

3. On meaſuring the diſtended Stomach of another Dog, I found its whole Surface equal to eighty ſquare Inches, which multiplied into thirty ſix, the Height of the Water in the

Tube, gives 2880 cubic Inches of Water or 104 Pounds Weight of Water, which preſſed againſt all Sides of the Stomach: And allowing the Area of the greateſt tranſverſe Section of the Stomach to be equal to thirty ſquare Inches, then the Preſſure of the Water againſt the Fibres of the Stomach in that Section, when it burſt, will be thirty nine Pounds. Which ſhews how greatly *Borelli* and *Pitcairn* were miſtaken, when they eſtimated the Force of the Fibres of the Stomach to be equal to 12951 Pounds; ſince we may with good Reaſon conclude that the Force of thoſe Fibres cannot in a live Animal be greater than the Force which will tear them aſunder as ſoon as dead. Neither can the Preſſure of the *Diaphragm* and of the Muſcles of the *Abdomen* on the Stomach, be in our utmoſt Strainings greater than a Weight of Mercury two Inches deep, and of the Breadth of all their Areas, as I have ſhewn under *Experiment* CXVI. *Vol.* I. p. 270. And that the Sum of the Compreſſure of the Muſcles of the *Abdomen* and *Diaphragm*, and alſo of the Stomach on its Contents, is not nearly equal to the Weight of two Inches Depth of Mercury, is evident from Appendix, *Experiment*

VII.

VII. of this 2d Vol. where it was found by a mercurial Gage fixed to the Nose of a large Pair of Smith's Bellows, that the most forcible blast of them will scarcely raise Mercury two Inches high in the Gage: And since such a blast of Wind is manifestly much greater than the most forcible puff of Wind, which is belched out of a much distended Stomach; 'tis hence evident that the Stomach even in that much distended Case does not compress what is contained in it with near that Force.

4. If we suppose the Surface of a full Stomach to be equal to eighty square Inches; and that its Contents are compressed by the Action of it, together with that of the *Diaphragm* and Muscles of the *Abdomen*, with a Force equal to one Inch depth, then the whole Pressure on its Contents will be equal to thirty nine Pounds, which is nearly the Weight of eighty cubic Inches of Mercury: But as this seems to be too great a Force by comparing the Velocity with which Wind rushes out of Bellows, when its Force is sufficient to raise Mercury an Inch in the Gage, so I believe half that Force, *viz.* about twenty Pounds would come nearer to the Pressure on the Aliments of a full Stomach.

5. Now so small a Compressure can have very little Effect in promoting the Digestion of the Aliments: Which is therefore with good Reason principally attributed to the Concurrence of several other Causes; such as Mastication and Comminution with the Teeth, and Mixture first with the *Saliva* (which is a Leven full of elastic Air) and afterwards with the Fluid which is in plenty separated from the Glands of the Stomach; and the active and expansive Principles of this thus macerated and moistened Mass, are by the Warmth of the Stomach disposed to some Degree of Ferment: Its Solution being also at the same time greatly promoted, not only by the peristaltic muscular Action of the Stomach, whereby, as also by the Help of the prominent *Rugæ*, or Foldings of the inner Coat of the Stomach, the Mass is more intimately mixed, and so better disposed to farther and farther Solution; but also principally by the incessant reciprocal Action and Reaction of the *Diaphragm* and Muscles of the *Abdomen*, which are each about twelve hundred in an Hour.

6. And the great Use of the thus Kneading the Contents of the Stomach to and fro,

in

in order to promote Digestion, is seen in the following Effect of it, *viz.* If in the Night the Stomach be loaded with a *Crapula* of indigested Food that offends it, it has been found by repeated Experience, that if the Person labouring under this Uneasiness, will for a considerable time draw in his Breath deep, almost to the Degree of sighing, the Kneadings of the Stomach being by these deep Depressions of the *Diaphragm*, double in force to what they usually are, the Stomach will by this means be much sooner exonerated of its troublesome Burthen.

7. When the Arteries of the Stomach of a Dog have been injected with Vermilion, and that Stomach has been blown up, in order to its being well dried; in this case the Water, which had as mentioned in *Experiment* IX. flowed thro' the Arteries and Veins in order to wash out the Blood, this Water would now flow plentifully from the Veins which were not injected with Vermilion; an Argument that there is less Blood in the Blood-vessels of the Stomach, when it is filled with Food than when empty. Whence as well as on other Accounts, the Stomach which is too much distended with Food, having then a less Quan-

tity of Blood flowing thro' its Veſſels, muſt not only be cooler, by reaſon of a more retarded Motion of the Blood, which we find in ſome Degree after ordinary Meals, but muſt alſo have much leſs Moiſture ſeparated from its Glands, at a time when the too great Maſs of Food requires an extraordinary Supply duly to be moiſtened; the Conſequence of which muſt be a more difficult and unkindly Digeſtion.

8. Since there is more Blood in the Veſſels of the Stomach when it is empty than when full, this greater Flow of Blood to the empty Stomach may probably contribute to increaſe the Appetite of Hunger; hence alſo it may be that the Digeſtion is ſtronger in Winter than in Summer, becauſe leſs being perſpired in Winter, the Quantity of Fluid retained in the Blood-veſſels being thereby greater, the Blood will be more vigorouſly impelled, as into all other Parts of the Body, ſo into the Veſſels of the Stomach, whence a greater Warmth, and alſo a more plentiful Secretion from the Glands of the Stomach, to promote the Digeſtion of the Aliment. The Increaſe alſo of the Appetite which is obſerved in the beginning of Pleuriſies, is attributed to the greater Flow of

 Blood

Blood to the Stomach, on its Paſſage being retarded in the *Pleura*.

9. I found the Gullet very dilatable with a ſmall Force of Water or Wind, and hence it is that when flatulent Wind is aſcending thro' it from the Stomach, the Gullet being thereby dilated, compreſſes the deſcending *Aorta*, where the Gullet is confined in its Paſſage between that and the Heart; and thereby for that inſtant the Blood is driven more forcibly up into the Head, which often cauſes a ſhort Swimming or *Vertigo*; an Inconvenience which the Flatulent are too frequently ſenſible of.

Experiment XXIV.

1. HAving as ſoon as a Dog was dead cut aſunder the *Duodenum* Gut juſt below the *Pilorus*, I poured warm Water into the *Duodenum*, thro' a glaſs Tube which was fixed to it; when the Water ſtood at two Feet perpendicular Height in the Tube, that Force impelled Water thro' the whole Length of the Guts, ſo as to flow out at the *Anus*: the *Fæces* in the *Rectum* made little Reſiſtance, being ſoft and not figurated.

2. But when in another Dog a Tube was fixed to the Gullet, and Water was poured in ſo as to burſt the Stomach and one of the Guts, yet there being hard *Fæces* in the *Rectum*, no Water paſſed thro' it.

3. From this Experiment we ſee how requiſite it is, in ſome colicky Obſtructions of the Bowels, to promote the Operation of Purgatives with Clyſters; without which, Purgatives may in ſome Caſes do more harm than good, while they increaſe the painful Diſtention of the Bowels, without being able to paſs and carry off the noxious Maſs.

4. I am very ſenſible that there was wanting in this Experiment, the periſtaltic Motion of the Guts, which in a live Animal promotes the Deſcent of their Contents. But 'tis to be feared that when in the *Iliack Paſſion*, there is a Stoppage in any part of the Guts, either by *Fæces* or diſtending Wind, if that Diſtention be ſuperior to the Force of the arterial Blood, it muſt neceſſarily ſtop the Courſe of the Blood in that part; which muſt conſequently both retard the periſtaltic Motion, and alſo cauſe an Inflammation; which if not ſoon prevented too often ends in a Mortification.

EXPERI-

Experiment XXV.

1. I Cut aſunder the *Rectum* of a Dog, and poured in warm Water thro' a Tube fixed to it; the Water gradually paſſed the Valve at the Cæcum, and flowed along the Guts, up to the *Pilorus:* The perpendicular Height of the Water in the Tube was five Feet.

2. In another Dog, I found the Valve at the *Cæcum* ſhut ſo cloſe that neither Water nor Air would paſs it, not even when cleanſed from the *Fæces.*

3. I fixed alſo a Tube to the *Rectum* of a third Dog, and poured in warm Water, till it ſtood twenty Inches high in the Tube; yet it did not paſs above ſix or eight Inches up the *Rectum* being hindred by the *Fæces,* which yet were not hard enough to be figurated: But when the *Fæces* were removed, then Water freely paſſed the annular Valve at the *Cæcum.* Hence we ſee how requiſite it is in ſome Caſes, firſt to get the *Fæces* below the Valve out by a Clyſter; after which the Effects of a ſecond Clyſter may reach much farther.

4. *Qu.* Have we not from hence as alſo from Numb. 1. of this Experiment, a hint

to

to try, at leaſt in ſome deſperate caſes, as in the *Iliack Paſſion*, whether any Relief might be had by applying a Clyſter thro' ſuch a perpendicular Height as ſhall be judged proper; this might probably paſs up to the affected part, and ſo poſſibly not only open a free Paſſage, but alſo by its proper Vertue, might aſſwage an Inflammation, and thereby prevent a Mortification. But the weaker the Patient is, of the leſs perpendicular Height ought the Tube to be, thro' which the Clyſter paſſes; elſe its Force may be too great for that of the arterial Blood; and ſo either by ſtopping, or at leaſt too much retarding its Motion thro' the Coats of the Guts, may endanger Life.

5. If Clyſters of different Qualities were thus injected into live Dogs from different perpendicular Heights, a more certain Judgment might be made, how far they might be uſed with Safety, and alſo whether any Benefit may reaſonably be expected from them.

AN

ACCOUNT

OF SOME

EXPERIMENTS

ON

STONES in the Kidnies and Bladder.

With an Inquiry

Into the NATURE of thoſe anomalous CONCRETIONS.

THE

INTRODUCTION.

1. I Am ſenſible that ſome may be apt to think, it ſavours too much of Vanity and Preſumption, for any one, after ſuch innumerable fruitleſs Reſearches of the ableſt Chymiſts, to attempt to find out a ſafe Diſſolvent of the Stone in the *Bladder*. The great Tenderneſs of thoſe Parts on the one hand, and the extream Hardneſs of many of the *Calculi* on the other, do with too much Probability, make the caſe ſeem deſperate. For after applying all imaginable chymical Preparations, it is found, that neither the alkaline, nor acid, the neutral, ſaline, ſulphureous, nor ſaponaceous Menſtruums, have any Effect on theſe Concretions; Spirit of Nitre, which is too corroſive, being the only thing which has hitherto been found effectually to diſſolve them. Yet in a Matter of ſo great Importance, to the Eaſe and Welfare of a conſiderable Part of Mankind, we ought not wholly

to

to deſpair of finding ſome proper and ſafe Diſſolvent. And we are hereto excited by the greateſt and moſt knowing Chymiſts, who do frequently encourage us, not to give over this Reſearch; being ſenſible there were Menſtruums powerful enough to diſſolve Metals and Stones, yet ſo mild as not to hurt, or offend the tendereſt part of the Body.

2. What more particularly induced me to make ſome few Attempts in this Matter, was, that when I was endeavouring ſome time ſince to analyſe the Air by many *Chymio-Statical* Experiments, among other Trials, I found that by Diſtillation, vegetable Tartar, *viz.* that of Wine, contained above five hundred times its Bulk of Air, which was a great deal more than I found, bulk for bulk, in any the fluid or ſolid Parts of Vegetables. This put me upon trying whether it was the ſame with animal Tartar, viz. *The calculus humanus.*

3. I was furniſhed with Variety of *Calculi* by the Favour of Mr. *Ranby*, *Surgeon to his Majeſty's Houſhold*, F. R. S.

EXPE-

EXPERIMENT I.

1. A Piece of *Calculus* that weighed two hundred and thirty Grains, and was ſomething leſs than $\frac{3}{4}$ of a cubic Inch in bulk, being diſtilled in a Gun Barrel, as deſcrib'd in *Vegetable Staticks*, *Vol.* I. *Exper.* LXXVII. *p.* 189. there aroſe from it, in Diſtillation five hundred and ſixteen cubic Inches of elaſtic Air, that is a Bulk equal to ſix hundred and forty five times the Bulk of the Stone, ſo that above half the Stone was raiſed by the Action of Fire, into elaſtic Air. The remaining *Calx* weighed forty nine Grains, that is $\frac{1}{4.69}$ part of the *Calculus*; which is nearly the ſame Proportion of *Calx*, that Dr. *Slare* found remaining, after the Diſtillation, and calcining two Ounces of *Calculus*, "one Ounce and three "Drams of which, he ſays, evaporated in "the open Fire (a material Circumſtance, "which the Chymiſts rarely enquire after) "of which we have no Account." *Philoſ. Tranſact. Lowthorp's Abridgment*, *Vol.* III. *p.* 179. the greateſt part of which was, we ſee by this Experiment, raiſed into permanently elaſtic Air.

2. Hence

2. Hence it was found that the *Calculus* and Tartar of Wine did yield more Air in Diſtillation, than any other Subſtances, whether animal, vegetable or mineral. And it was remarkable that a greater Proportion of this new raiſed Air from theſe two Subſtances, was reſorbed and loſt its Elaſticity, in ſtanding a few Days, than that of any other Parts of Animals or Vegetables, which are ſtrong Symptoms that the *Calculus* is a true animal Tartar. And as there was very conſiderably leſs Oil, in the Diſtillation of Rheniſh Tartars, than there was in the Diſtillation of the Seeds and ſolid Parts of Vegetables; ſo I found that this *Calculus* contained much leſs Oil, than the Blood or ſolid Parts of Animals. But ſome Stones which were taken out of a human Gall-bladder yielded in Diſtillation more Oil, and much Air.

3. A ſmall Stone of the Gall-bladder, which was of the Size of a Pea, was diſſolved in a *Lixivium* of *Sal Tartar* in ſeven Days, which *Lixivium* will alſo diſſolve Tartar, yet it will not diſſolve the *Calculus*, tho' I boiled a hard Piece in it for ſeveral Hours.

4. A Quantity of *Calculus* equal to one half of what was diſtilled, *viz.* a hundred

and fifteen Grains, did, when a cubic Inch of Sp. of Nitre was poured on it, diſſolve in two or three Hours, with a large Froth, and generated forty eight cubic Inches of Air; none of which loſt its Elaſticity, in ſtanding many Days. A like Quantity of Tartar, having Spirit of Nitre poured on it, was at the ſame time diſſolved, but no elaſtic Air was generated, notwithſtanding Tartar abounds ſo much with Air.

5. Small Pieces of Tartar and *Calculus* were in twelve or fourteen Days diſſolved in Oil of Vitriol; the like Pieces of Tartar and *Calculus* were diſſolved in a few Hours by Oil of Vitriol, into which there was gradually poured near an equal Quantity of Spirit of Harts-horn made with Lime, which Mixture made a conſiderable Ebullition and Heat.

6. Tho' the remaining *Calx* of the Diſtillation of Tartar, runs *per deliquium*, and has therefore *Sal Tartar* in it; and tho' the *Calx* of diſtilled *Calculus*, does not run *per deliquium*, and has conſequently no *Sal Tartar* in it; yet it cannot thence be inferred, that the *Calculus* is not a tartarine Subſtance; becauſe *Sal Tartar* it ſelf, when mixed with an animal *Calx*, diſtills all over, ſo that the *Calx*

will not afterwards run *per deliquium.* By the great Similitude there is therefore in ſo many Reſpects, between theſe two Subſtances, we may well look upon the *Calculus* as a true *Animal Tartar*, and doubtleſs gouty Concretions are the ſame.

7. *Animal Tartar* I would call it in Contradiſtinction to *Vegetable Tartar*; for as animal and vegetable Subſtances greatly differ in their Salts and Sulphurs when chymically analyſed, ſo do alſo their reſpective Tartars: But then they are found exactly to agree in theſe remarkable Properties, *viz.* That they are both formed not barely as a Sediment at the bottom of their Liquors; but being equally expanded and ſeparated from all parts of their Fluids, they adhere uniformly to the Sides of their containing Veſſels in the Form of a hard Cruſt: Thoſe Particles which are immediately next the Sides of the Veſſel being firſt attracted and adhering to them. And as it is a known and wonderful Property of Fluids, to diffuſe what is mixed with them equally in every part, ſo when part of theſe incruſting Particles are attracted out of the Fluids, the remaining Particles equally diffuſing themſelves, a new Succeſſion of them is thereby brought within the Attraction of the already

already incruſting Particles, and ſo on ſucceſſively: Otherwiſe the Particles which float in the Fluid at ſome Diſtance from the Sides of the Veſſel, could not be attracted thither, for this Attraction of Coheſion acts only near. Further, theſe Tartars are both obſerved to be depoſited in greateſt Plenty, when the Sides of the Veſſels have been already incruſted: They agree alſo in this that the animal and vegetable Fluids depoſit their incruſting Matter, in greateſt Plenty when they are in an attenuated State, and the more attenuated they are, the harder will their Incruſtations be. They both agree alſo in this, that they yield in Diſtillation more Air than any animal or vegetable Subſtance. With good reaſon therefore has the *Calculus* been called a Tartar by many. And as the *Germans* call the Tartar of Wine, Wine Stone, ſo the *Calculus* may be called Urine Stone; as alſo the Incruſtations from mineral and other Waters may be called Water Stones: But as they are all formed in the ſame manner, and have ſeveral of the ſame Properties in common, ſo they may not improperly be called the Tartars of thoſe ſeveral Fluids.

8. From the great Quantities of Air, that are found in theſe Tartars, we ſee that unelaſtic Air Particles, which by their ſtrongly attracting Property, are ſo inſtrumental in forming the nutritive Matter of Animals and Vegetables, is by the ſame attractive Power, apt ſometimes to form anomalous Concretions, as the Stone, *&c.* in Animals, eſpecially in thoſe Places where any animal Fluids are in a ſtagnant State, as in the Urine and Gall-Bladders. They ſtrongly adhere alſo to the Sides of Urinals, *&c.* the like tartarine Concretions are alſo frequently formed in ſome Fruits, particularly in Pears; but they do then eſpecially coaleſce in greateſt Plenty, when the vegetable Juices are in a ſtagnant State, as in Wine Veſſels, *&c.*

9. This great Quantity of attracting, unelaſtic Air Particles, which we find in the *Calculus*, ſhould rather encourage than diſcourage us, in ſearching after ſome proper Diſſolvent of the Stone in the Bladder, which in the *Analyſis* of it is found to be well ſtored with active Principles, ſuch as are the great Agents in Fermentation. For Mr. *Boyle* found therein a good Quantity of volatile Salt, with ſome Oil, and we ſee by theſe Experiments, that there

there is ſtore of unelaſtick Air Particles in it, which are raiſed in Diſtillation into an elaſtic State, at the ſame time that the volatile Salt ariſes in white Fumes, an Argument that they are intimately combined with each other in the *Calculus.*

10. And it was this Conſideration, which put me upon making ſome Attempts, to try by Variety of fermenting Mixtures, whether the brisk Action of ſome Fermentations, might not poſſibly ſo ſhake and rouſe theſe Air Particles, as to make them fly off into an elaſtic State, and thereby diſſolve the Union of the Parts of the Stone. We find that Spirit of Nitre acts in this manner on the *Calculus*, in the Diſſolution of it; for we ſee in *Experiment* I. *Numb.* 4. that a hundred and fifteen Grains or about $\frac{1}{3}$ of a cubic Inch of *Calculus*, when diſſolved in Spirit of Nitre, generated forty eight cubic Inches of Air, with a large Froth, a Quantity equal to a hundred and forty four times the Bulk of the *Calculus.*

11. From the few Tryals I have hitherto made, I have found a fermenting Mixture, which by its vigorous Action, not only throws off elaſtic Air from many human *Calculi*, but alſo very ſenſibly waſtes and diſſolves ſeveral of

thoſe which are of a leſs compact Texture; eſpecially the Gravel Stones, which are not generally ſo hard, as Stones which have had a longer time to harden in the Bladder; yet I have met with ſome of theſe Gravels ſo hard as not to be diſſolved by this Mixture.

12. Tho' I have not herein ſucceeded ſo far, as to encourage any one to attempt the diſſolving therewith Gravel Stones in the Bladder: Yet I thought it might not be improper to give an Account of the ſmall Progreſs I had made herein; ſince it might poſſibly be a Step, towards the happy and important Diſcovery, by exciting and engaging others in the ſame Purſuit; who among the infinite Variety of Menſtruums that may be compounded, may haply hit upon ſome, which may more eaſily diſſolve moſt Gravel Stones at leaſt, if not the larger and harder *Calculi.* If we could make no farther Advance than this, it would be of conſiderable Benefit to Mankind, by taking away, when it firſt falls into the Bladder the *Nucleus* on which larger and harder Stones are in time formed. And this would be effected, if with a few Injections of a ſafe and proper Menſtruum, we could only diſſolve a ſmall part of a large Gravel Stone, lately fallen

fallen from the Kidnies, ſo as to make it fit to paſs off thro' the *Urethra*; which it would alſo do with more eaſe to the Patient, by having thereby, its Surface and ſharper Points ſoftened and made mucilaginous, and leſs ſtimulating. This is what the fermenting Mixture I am going to give an Account of, will do on ſome of the ſofter Stones, but not without too many repeated Injections, to make it practicable with Safety to the Patient.

13. That I might with the greater Accuracy and Readineſs vary and adjuſt, as occaſion required, the ſeveral Proportions of the fermenting Mixtures, I divided the Capacity of ſeveral Glaſs Veſſels, out of which I poured the Liquors, into cubic Inches; making Marks with a File on the Outſide of the Glaſſes, at each Diviſion. I divided alſo the Capacity of a large Tube into quarter cubic Inches, it was half Inch Diameter, and ſealed at one end. I made alſo ſeveral Diviſions on a ſhort Tube which was $\frac{1}{4}$ Inch Diameter, the Capacity of each Diviſion containing ten Drops of Oil of Sulphur; ſo that by dipping one end of this Tube in Liquor to any of theſe Marks, and then ſtopping the other end with my Finger, I could readily take up 10, 20, 30, 40 or 50

Drops, or any intermediate Number.

14. I will now give a ſhort Account of ſome of the principal Experiments I made; in doing of which I did not confine myſelf to ſuch mild Mixtures, as might probably not be injurious to the Bladder; but choſe to begin with the ſtrongeſt fermenting Mixtures, in hopes that if I ſhould find any of theſe would diſſolve the *Calculus*, by gradually weakening and lowering ſuch Mixture, I might poſſibly bring it to ſuch a Degree of Mildneſs, as might make it not injurious to the Bladder, and yet retain in ſome meaſure its diſſolving Quality. Or if I ſhould not be ſo happy as to ſucceed ſo far, yet I thought it moſt probable, that I might find Diſſolvents, among theſe ſtronger Mixtures, which might at leaſt give ſome farther Inſight into the Nature of the *Calculus*.

Experiment II.

1. A Cubic Inch of Oil of Vitriol, and double its Quantity of Water, when mixt together, made ſo hot a Ferment that I could hardly bear to hold my Hand at the Bottom of the Bolthead; yet it had no Effect on a Piece of the hardeſt kind of *Calculus*. And

it

it was the ſame when the Heat and Ferment was renewed by throwing in ſome Filings of Iron.

2. The like Proportions of Oil Vit. and Water, with ſeveral kinds of powdered vitriolic Stones or *Pyrites*, made violent Ferments, but had no Effect on this very hard *Calculus.*

3. It was the ſame with Oil of Vitriol and other acid Spirits, when poured on ſeveral alkaline Bodies, ſuch as powdered *Belemnites*, *Aſteria*, Coral and Oyſter-ſhell, *&c.*

4. But Oil of Vitriol mixed with unrectified Spirit of C. C. tho' it would not diſſolve the abovementioned very hard Piece of *Calculus*, yet with ten or twelve freſh Mixtures, it rotted and diſſolved ſeveral pretty thick *Laminæ* or Cloves of other *Calculi*; which tho' not of ſo compact a Texture as the other, yet were ſo hard that I could not make any Impreſſion on them with my Nail: But this was too pungent a Mixture to give any Hopes that the Bladder can bear any Degree of it.

5. Spirit of Rye-bread being found by Chymiſts a Diſſolvent ſo powerful as to diſſolve ſeveral kinds of Stones and hard Subſtances, and yet withal ſo mild, that it may with great Safety be held in the Hollow of the Hand; I

prepared

prepared a good Quantity of it, both unrectified and rectified, and made it one in a great Variety of briskly fermenting Mixtures, in hopes that by thus putting its Parts in a brisk Motion, it might have made ſome Impreſſion on the *Calculus*, but it had no Effect.

6. I made a Preparation of Tartar of Vitriol, by mixing one part of Oil of Vitriol, with twice its Quantity of hot Water, in which were Pieces of *Calculus* and Tartar; the *Calculus* emitted Bubbles, but not the Tartar: Then I poured in gradually *Ol. Tart.* when both *Calculus* and Tartar emitted for ſome Minutes a great Quantity of Bubbles. The Tartar was almoſt at once diſſolved, and the *Calculus* was very rotten and brittle, but it was not of the very hard kind. *Sal Tartar* which is a fixt Alkaly, being leſs corroſive than Spirit of Harts-horn which is a volatile Alkaly.

Experiment III.

1. I Made a Solution of it in Water, in the Proportion of an Ounce of *Sal Tartar* to four of Water; and made many brisk Fermentations, with ſeveral Portions of this al-

kaline

kaline Mixture, by pouring in of the ſtrongeſt acid Spirits, as Spirit of Nitre, Spirit of Salt, Spirit and Oil of Vitriol, and Oil of Sulphur. I found Oil of Vitriol and Oil of Sulphur moſt effectual to my Purpoſe, and of theſe two, I choſe rather to make uſe of Oil of Sulphur than Oil of Vitriol, as being the purer Acid of the two, and leſs noxious to animal Bodies than Oil of Vitriol.

2. I found after the Mixture of a great Variety of Proportions of theſe Liquors, that the following were the propereſt for my Purpoſe, *viz.* one cubic Inch of Water, one third of a cubic Inch of Solution of *Sal Tartar*, and twenty five or ſometimes thirty Drops of Oil of Sulphur. Alſo ſix cubic Inches of Water, $\frac{3}{4}$ Cub. of the Solution of *Sal Tartar*, and fifty Drops of Oil of Sulphur.

3. Theſe Proportions fermented briskly, and made Air Bubbles ariſe very faſt from the *Calculi* for eight or ten Minutes, as would alſo ſeveral other Proportions, tho' I did not find any ſo effectual as theſe, which after ſeveral freſh Affuſions of them, would diſſolve ſome *Calculi* which were hard to the Touch: They would alſo diſſolve ſeveral Gravel Stones, tho' not all; nor had they any Effect on many of the hardeſt *Calculi*.

4. If

4. If Air do not ariſe briskly from the *Calculus*, upon pouring on any of theſe Mixtures, the Rule to adjuſt the Proportions accurately, is to drop in a few Drops of Oil of Sulphur; if this Addition make more Air ariſe from the *Calculus*, it is a ſign ſome of it was wanting, but if more Air does not ariſe, then it is a ſign that more Solution of *Sal Tartar* is wanting.

5. It fermented beſt, when half the Water was poured on the Solution of *Sal Tartar*, and the Spirit of Sulphur was dropped into the other half, and then both Mixtures poured together. Warm Water was better than cold, tho' it fermented longer cold.

6. When there was a double Quantity of Oil of Sulphur, I have not found it more powerful in diſſolving, and when the Solution of *Sal Tartar* was much ſtronger the Ferment was leſs.

7. This Liquor has no Effect on the *Calculus* after the Ferment is over, as I have found by laying ſeveral *Calculi* to ſoak in it for a whole Year. So that the Effect it has on the *Calculus* while it is fermenting, ſeems not to depend on the Fitneſs of the Particles of the Menſtruum to enter the Pores of the *Calculus*, but rather to certain harmonic Proportions

portions between the Vibrations of the fermenting Liquor, and the Tone or Degree of Tenſeneſs of the Parts of the *Calculus*: Juſt as when two Strings are equally tenſe, the ſtriking of one will make the other ſound; or as I have often obſerved the different Pipes of an Organ, will make different Boards vibrate, according to the Uniformity there is, between the Tenſeneſs of the Fibres of the ſeveral Boards, and the Tone of the different Pipes.

8. And in like manner we may not unreaſonably ſuppoſe, that when there is the like Uniformity between the Vibrations of the fermenting Liquor, and thoſe of the Parts of the *Calculus*, that the *Itus* and *Reditus* of theſe Vibrations, increaſing in this Caſe each other's Force, ſome parts of the *Calculus* are thereby thrown off into elaſtic Air. In Confirmation of this Conjecture, I have obſerved Air to ariſe briskly from a *Calculus* with twenty or ten Drops of Oil of Sulphur, and yet with fifteen Drops little or no Air aroſe, notwithſtanding the Proportion of the other Ingredients of the fermenting Mixtures, was the ſame in all three Caſes.

EXPERIMENT IV.

1. A *Calculus* which weighed three hundred and fourteen Grains, after forty nine fresh Affusions of the abovementioned dissolving Menstruum was wasted a hundred and thirty four Grains; but the remaining *Nucleus* of this *Calculus* was so very hard, that this Liquor had no Effect on it.

2. I dissolved also several other of the softer kind of *Calculi* and the broken Shells or Cloves of others; but there were many of the harder kind, which this Liquor had no sensible Effect upon.

3. When a large *Calculus* was sawn asunder, and immersed in the fermenting Liquor, I observed the Air to rise in much greater Quantity, from the softer inward part of the *Calculus*, than from the hard polished Surface.

4. As for Gravel Stones I was obliged to several Persons who were so good as to furnish me with 'em, which I made the following Experiments upon, *viz.*

5. Some small Grains of a redish yellow gritty Gravel were crumbled into coarse Sand with seven Affusions of one cub. Water, $\frac{1}{3}$ cub.

cub. of Solution of *Sal Tartar*, and twenty five Drops of Oil of Sulphur; the Air which roſe from the Gravel, adhering to it in large Bubbles, made the Gravel frequently riſe to the Top of the Liquor.

6. I put into the ſame Menſtruum a Clove or Fragment of a Scale of a larger *Calculus*, which was voided the Day before, it was $\frac{1}{16}$th part of an Inch thick, and $\frac{1}{8}$ broad; of an aſh Colour, ſo hard that it was difficult to break away its Edges with ones Nail. After thirty ſix Affuſions of this Liquor, it was rotten as Dirt, tho' it retained its Shape.

7. Three hard rediſh Gravels, of a third Perſon's, which were as big as large Shot, were after eleven Affuſions of the ſame Liquor, conſiderably more brittle, their Surface rotten, and much diminiſhed, and after twenty ſix more Affuſions, they were waſted to the Size of Pins-heads.

8. I layed two other Gravel Stones of the ſame Perſon's, but ſomewhat leſs than the former, to ſoak in Urine for twenty four Hours, and then poured on eight Affuſions of the diſſolving Menſtruum: The following Night they were laid to ſoak again in Urine, and the next Day I poured on ſixteen more Affuſions;

after which one was wasted and the other very rotten; so the soaking in Urine does not hinder Dissolution.

9. I dissolved in the same manner the Gravel of three other Persons, which were either of an ash Colour, or such as seemed to be composed of a coarse redish sandy Grit.

10. But the larger Gravel of two other Persons, which had probably lain much longer a hardening in the Kidnies or Bladder, and were covered with a kind of hard Polish, had very little Impression made on them by this Liquor; yet some Gritt came off the Surface of one of these after eleven Affusions; and after thirty more Affusions, each end was soft and crumbled. But this Liquor made no Impression on the Gravel of the other Person.

11. This Liquor dissolved a Piece of *Tartar* in five Affusions, but a like Piece of Tartar lay thirty Hours in the same Liquor, before it was dissolved, when there were no repeated Affusions.

12. That all the Air Bubbles which arise during the Ferment do not arise from the *Calculus*, is plain, because some arises in that part of the Glass Vessel where there is no *Calculus*, these Bubbles therefore must arise from the

the fermenting Mixture, for *Sal Tartar* by *Experiment* LXXIV. *Vol.* I. *p.* 184. contains a good Quantity of Air. Yet far the greateſt Quantity of Air ariſes from the *Calculus*, as is evidently to be ſeen, when two like Quantities of this Liquor are poured at the ſame time into two Glaſs Veſſels, which have, one of them, a *Calculus* in it, and the other none.

13. Tho' this Menſtruum is far from being efficacious enough to encourage any one to make the Tryal on human Bodies, it requiring as we ſee too many repeated Injections to diſſolve ſome of the ſofter kinds of *Calculi*; yet I thought it not improper to try whether the Bladder could bear a Liquor with this Degree of Acidity in it; it being ſo mild that it may be taken into the Mouth without Inconvenience, tho' it ſets an auſtere Roughneſs on the Teeth.

Experiment V.

1. I Injected therefore at three ſeveral times thro' a Tunnel and Tube about half a Pint of this Liquor into the Bladder of a Dog, after which he did not ſhew any Signs of Uneaſineſs: But when another time a Pint

and half of near double the Strength of the former was injected, he ſeemed uneaſy as if troubled with the Strangury, but the uneaſy Symptoms went off in half an Hour. Some Days after I killed the Dog, but could not perceive on opening him, that the injected Liquor had done any harm to his Bladder.

2. Theſe Injections are eaſily made into a Dog's Bladder, by paſſing a Catheter directly into it, thro' an artificial *Fiſtula* made in the *Perinæum*, as directed by Mr. *John Douglas*, Surgeon, F. R. S. in the *Philoſoph. Tranſ. Numb.* 399.

3. I injected alſo the ſame Liquor twelve ſeveral Days into the Bladder of a Bitch, after which ſhe ſometimes ſhewed ſome Uneaſineſs for a little while and ſometimes not. She continued very lively and brisk long after; but ſome Months after in the following Summer, ſhe was in Copulation Dog-drawn as it is called, *i. e.* the Vagina was drawn out, which cauſed her Death, whence we may conclude, that its Fibres were hardened and contracted by the acid Spirit of Sulphur, and it may probably have had the like Effect on the Fibres of the Bladder. It is therefore by no means adviſeable

adviſeable to attempt the Uſe of it in a human Bladder; neither do I here give an Account of it with any ſuch View; my Deſign herein being only to ſhew, that this is at leaſt ſome Advance towards diſcovering a ſafe diſſolvent for moſt Gravels and ſome of the ſofter kinds of *Calculi*; but as to the harder kinds, on which nothing has hitherto been found to have any effect towards a Diſſolution, except *Aqua-fortis*, there is little Hopes of ever finding a ſafe Diſſolvent for them. But as to the ſofter kinds there are ſeveral mild acid Mixtures that will diſſolve them, when laid to ſoak in them for a few Days.

4. I tried to make this Liquor more mild by mucilaginous Mixtures; ſuch as Solution of Gum Arabick, and Decoction of Comfrey Root; but I did not find it ſignify'd any thing; it ſerved only to increaſe the Froth in the Effervеſcence, which was alſo the Inconvenience of uſing Urine inſtead of Water: For I diſſolved ſeveral Pieces of *Calculus*, with Urine Solution of *Sal Tartar* and Oil of Sulph. ſo that a little Urine in the Bladder would be no Impediment to the Operation of this Liquor.

Experiment VI.

1. THinking it might poſſibly be of uſe in Experiments of this kind, to have a continual Flow of Liquor in and out of the Bladder, I directed an ingenious Artiſt to make me a double *Catheter*, the Cavity of which was divided lengthwiſe by a thin Partition, into two ſeparate Channels which ended in two divaricating Branches. To one of theſe Branches was faſten'd an Ox's Ureter, or the Windpipe of a Gooſe or Turkey, which by means of a Glaſs Tube either communicates with or has Water flowing from a capacious Veſſel, placed three Feet above the *Catheter*: So the Liquor as it flows from the Veſſel, runs thro' the one half of the *Catheter*, and flows into the Bladder, and then returns out of the Bladder thro' the other half of the *Catheter*.

2. Thro' this Inſtrument I cauſed twenty three cubic Inches of the diſſolving Liquor to flow in and out of the abovementioned Bitch's Bladder in a continual Stream: And after that for four Hours and a half, there ran inceſſantly in and out in the ſame manner a Stream

Stream of Water of the Warmth of Urine in quantity about nine hundred cubic Inches or three Gallons Ale Meaſure, and this without the leaſt Harm or Inconvenience to the Bitch, that I could perceive; ſometimes when the Bladder was pretty full, the Water flowed out between the *Sphincter* and the *Catheter*, and then ceaſed to flow till it had filled the *Bladder* to the ſame Degree of Fulneſs again.

3. Dr. *Keill* in his *Medicina Statica Britannica*, *p.* 14. obſerves that the Quantity of Urine which he made in twenty four Hours Day and Night, was about thirty nine Ounces, of which he ſays twenty one Ounces may be allowed for what paſſed off in twelve Hours Day, which is near at the rate of two Ounces in an Hour, in the Day-time; which ſuppoſing it to be at that rate, then there having paſſed thro' the double *Catheter*, nine hundred cubic Inches in four Hours and a half; that is at the rate of two hundred cubic Inches, or a hundred and thirteen Ounces in an Hour: Then the Proportion the Urine, which will flow into the *Bladder* in that time, will bear to the Water, which paſſed thro' the *Catheter* in the ſame time, will be as one to fifty ſix, which proportion may be much di-

miniſhed, by increaſing the perpendicular Height; and conſequently the Force of the Water which flows thro' the *Catheter*, as alſo by abating on ſuch an Occaſion the Quantity of Drink that is taken in. This ſmall Proportion of Urine to Water may probably very little diſturb the Effect of any Liquor, that may by this means be made to paſs into the Bladder; whether for diſſolving of the *Calculus*, if any ſuch Liquor ſhould ever be diſcovered, or whether it be for any medicinal Purpoſe to cure any Diſorders in the *Bladder*; in which caſes this Inſtrument may poſſibly be ſometimes of uſe.

4. But if upon Tryal it be found that the Canals of this double *Catheter*, are too ſmall, then it may perhaps be a better way to paſs any Liquor into the *Bladder*, thro' a ſmall or middle ſized common *Catheter*, thro' which it would flow more plentifully in, and alſo paſs out more freely, between the *Sphincter* and the *Catheter*, when the *Bladder* is full to a certain Degree, whereby any groſs, ſlimy or fæculent Matter would more eaſily be diſcharged from the *Bladder*, than thro' the narrower Paſſage of a double *Catheter*.

EXPERI-

Experiment VII.

1. I Pounded ſeverally the three following hot alkaline Plants, viz. *Onions*, *Scurvy-graſs* and *Horſe-radiſh* Roots, and put the Pulp into three Pots, in the middle of which I put very hard *Calculi* which were all taken out of the ſame Perſon. I preſſed the Maſh hard down, and ſet the Pots in a hot Bed for thirteen Days.

2. The *Scurvy-graſs* and *Horſe-radiſh* had no ſenſible Effect on their *Calculi*: But the Surface of that which was put into the *Onions* was ſo ſoft that I could ſcrape it off with my Nail; and it was the ſame when I put a like *Calculus* into Onion Juice mixed with Water, which was kept warm in a Chimney Corner for fifteen Days; in which Liquor the rediſh gritty Gravel of two other Perſons was at the ſame time diſſolved.

3. Hence *Onion* Juice ſeems to have ſome conſiderable Efficacy in diſſolving the *Calculus*: And where frequently eaten ſhould, if not waſte, at leaſt prevent the Increaſe of, the *Calculus*.

Experiment VIII.

1. IN the Hiſtory of the *Royal Academy* of Sciences at *Paris*, An. 1720. there are Accounts of ſeveral *Calculi* being diſſolved by laying them many Days in Water, ſooner or later according to their different Degrees of Hardneſs.

2. I took ſome of the abovementioned, aſh-coloured, and round rediſh Gravel Stones, and put ſome of them at the ſame time into cold and warm Water, and found thoſe in warm Water diſſolv'd much ſooner than the other, they being covered with a white *Mucus*.

3. I laid ſeveral Gravel Stones in a little Rill of warm Water for fourteen Days and a good part of thoſe Nights: They had ſeveral of them a white *Mucus* over them; but they did not diſſolve ſo faſt as thoſe in ſtagnant warm Water; the Reaſon of which might be that this running Water was but half ſo warm as the abovementioned ſtagnant Water; and they were beſides ſome part of each Night cold, when the Water ceaſed to run.

4. I poured into a *Florence Flask* thirty nine cubic Inches of Water, and one of freſh U-

rine,

rine, and put therein the Gravel of ſeven different Perſons, and then ſet the Flask in hot Dung, where it had a Heat equal to that of Blood; but they had very little *Mucus* on them in ſix Days, ſo that this ſmall Proportion of Urine ſeems to be ſome hindrance to Diſſolution.

5. What I had principally in view in making theſe laſt mentioned Experiments, was to ſee whether we might have any reaſonable Expectation of waſting in ſome Degree, Gravel Stones, by a continued Series of Diuretics; but they give us very little Encouragement to hope for any other Succeſs from thence, than barely the waſhing down of the Gravel if not grown too large. It ſeems indeed probable that while Diuretics are uſing, the Gravel or Stone may make a very little if any Progreſs in increaſing in bulk, becauſe at that Time, the Urine is more diluted and leſs rancid and urinous; and conſequently not ſo much charged with tartarine Particles: For it is obſerved that as thoſe of a hot Temperament, and of the hardieſt Conſtitutions, have the moſt rancid Urine, ſo they are generally more ſubject to the Stone than others: Not only becauſe they are ſuppoſed to perſpire the watry

watry Matter more plentifully, but alſo becauſe their Urine is more highly alkalized, attenuated and digeſted, and its tartarine Parts thereby ſubtilized, and rendered leſs mucilaginous than the Urine of thoſe of a leſs robuſt and more lax Conſtitution of Fibres, which is probably a principal Reaſon why Women are much leſs ſubject to the Gravel or Stone than Men.

6. As Fermentation breaks and diſſolves the mucilaginous Texture of vegetable Fluids, ſo is the like Texture of animal Fluids diſſolved in proportion to the Degree of Digeſtion which they undergo. For as all Degrees of Ferment in vegetable Fluids have a Tendency from Viſcidity to Acidity: So all Degrees of Digeſtion, from its firſt beginning in the Stomach, thro' its whole Courſe, have a Tendency to Putrefaction: But in a healthy State of an Animal, this Tendency is ſtopped at a certain Degree, by the ſweet Emulſion of freſh Nutriment, without a conſtant Supply of which, the animal Fluids would ſoon tend to a fatal Degree of Putrefaction, and Putrefaction diſſolves Viſcidities. And accordingly it is obſerved that Urine depoſites its Tartar more freely to the Sides of Urinals, when it has ſtood ſome time to putrifie, and to become

thereby

thereby leſs viſcid and more dilute. Thus alſo *Muſt* or the unfermented Juice of Grapes yields no Tartar; nor do Wines depoſite their Tartars to the Sides of Veſſels, while they are in a thick, turbid, mucilaginons State; but when by a farther Degree of Ferment, they are ſo much diluted as to fall fine, then the tartarine Particles being diſintangled, do plentifully adhere to the Sides of Veſſels, ſo as to form thereby a hard Cruſt. And as the Tartar of Wine is obſerved to be the harder, the finer and more attenuated the Wine is, ſo probably the more digeſted and attenuated the Urine is, the *Calculus* will be proportionably the harder; and frequently to a very great Degree of Hardneſs, notwithſtanding it lays conſtantly in Urine; thus the Mortar of Walls which are ſo ſituated under ground as to be never dry, is often obſerved to be harder than that which lays dry; and thus the Bones of Animals, and the woody Subſtance of Trees harden in a continually moiſt State: And as Naturaliſts have obſerved that Bones of Animals are more compact and harder in hot than in cold Climates, ſo 'tis probable that *cæteris paribus* the *Calculus* may be harder in hot than in cool Conſtitutions; agreeably to what

Dr.

Dr. *Frederic Hoffman* obſerved of the ſtony Incruſtations in the Caroline Bath, which are hardeſt and redeſt, but in leaſt Quantity at the Mouth of the Spring when the Water is hotteſt; but as the Water grows cooler at a farther Diſtance from the Spring Head, there the Incruſtation is greater in Quantity, but ſofter, more friable, and like Sand. *Diſquiſitio Phyſico-Medica de Thermis Carolinis.*

7. Nature ſeems to have given us a hint, that ſoft mucilaginous Things are good to prevent the Growth of the Stone, by the Care ſhe has taken to line the Ureters and Bladder with the like Subſtance, which is ſeparated from its Glands; which ſerves not only to prevent the Bladder's being injured by the Acrimony of the Urine, but alſo to hinder the Adheſion of the tartarine Particles of the Urine, which are then found to adhere to the Bladder, when this mucilaginous lining is by the *Calculus* rubbed off: And a *Calculus* Sand is ſometimes found adhering to the whole Subſtance of the Bladder, which grows thereby thick, hard and ſchirrous. And daily Experience teaches us that the Tartar of Urine conſtantly adheres to Urinals and other Veſſels it is contained in; and would doubtleſs do the ſame to the Sub-

ſtance

ſtance of the Bladder, were it not defended with a ſlimy Coat. And accordingly it is obſerved by modern Phyſicians, that the more mucilaginous the Urine itſelf is, the leſs apt it is to breed the Stone: No wonder then, that balſamic, lubricant and mucilaginous Liquor, ſuch as Milk, ſoft unctuous Ale, Water-Gruel, Barley-Water, and Liquors made of or mixed with Honey, *&c.* are found to be good Preventives of the Growth of the Gravel or Stone.

8. And as toaſted Bread, roaſted, broiled and fryed Meats of all kinds, are obſerved to be apter to breed the Stone than boiled, this is doubtleſs owing to the ſame Cauſe, *viz.* that they are leſs mucilaginous than boiled Meats; for the Surface of theſe Meats being imbrowned, that is in ſome Degree burnt, the mucilaginous Texture of theſe burnt Parts is thereby ſpoiled, and the Coheſion of the tartarine Air Particles (many of which according to the different Degrees of burning are thrown off into an elaſtic State) is to ſuch a Degree broken as to leave them more at liberty to form in this their detached unelaſtick State.

9. Hence

9. Hence we ſee how greatly the Antients were miſtaken who generally attributed the Growth of the *Calculus* to mucoſe, pituitous Matter, which is ſometimes in great Plenty ſeparated from the ſcrophulous Glands of the Bladder, but is not found to harden into a *Calculus*.

10. It was the general Opinion of the Learned in *Beverovicius*'s Time (who publiſhed what they wrote to him on that Subject about a hundred Years ſince) that the Matter of the *Calculus* was a *pituitous* viſcid Matter, cauſed by the ill diſpoſed Kidney, and hardened by the Heat of the Bed. But *Helmont* in his Treatiſe *de Lithiaſi*, juſtly explodes this Notion, and denies the *Calculus* which he calls *Duelech* to be owing to any viſcid Matter for the following Reaſons, *viz.*

11. He ſays the viſcid Matter, which is ſometimes found in thoſe who have a Stone in the Bladder, ariſes from the Mucilage which the Stone rubs off the Bladder; for when the *Calculus* is cut out of the Bladder, there is no more viſcid Matter in the Urine: That if it aroſe from this *Mucus*, the Bladder would ſoon be filled with a *Calculus*: That this *Mucus* dried, makes only a little Chalk Stone,

like that of the *Mucus* of the Noſe when dried: And that it cannot incruſt in the Bladder to form a *Calculus*; whence he concludes that it does not ariſe from this *Mucus*, but from a part of the Urine which adheres to Urinals.

12. Tho' he filtrated Urine thro' Linen, yet it ſtill depoſited its Tartar, whence he concludes that the Tartar was not formed when the Urine was firſt voided; or if it had paſſed the Linen in the Form of Sand, then it would have precipitated, like Sand or Sediment, to the bottom of the Urinal, and not have adhered to the Sides at equal Diſtances; becauſe it would have wanted, he ſays, a Glue whereby to have adhered to the Sides.

13. And ſince, as he further obſerves, this Sand is no where glutinous, but where it adheres to the Veſſel, it is, he ſays, hence evident, that this Sand adheres at the inſtant it is formed: And this Sand is formed and adheres to the Veſſel long after the Urine is made, when it begins to putrify, the very inſtant before it fixes. To which more and more is continually added. And in the ſame manner he concludes the *Calculus* to grow. He obſerves this Tartar to adhere ſooner and in greater Plenty

Plenty to a Veſſel already incruſted with it, than to a clean Veſſel, *viz.* becauſe the fixt Tartarine Particles attract more ſtrongly than thoſe of the Sides of the Veſſel: An uncomfortable Obſervation for thoſe to reflect on, who have a *Calculus* once begun in them.

14. He found that even diſtilled Urine depoſited a Tartar; the Reaſon he gives why Urine long detained in the Bladder does not depoſite its Tartar to the Sides of the Bladder, as in a Pot, is becauſe in the Bladder it is not ſo ſoon diſpoſed to putrify, and that Tartar will not adhere ſo well to the Bladder as to a Pot.

15. *Helmont* finding the Ignorance and Incogitancy of others on this Subject, ſays, he gave away as many Books, as were worth two hundred Pieces of Gold, and wiſhed he had burnt them; for Books gave him not the leaſt Light. A mortifying but ſtrong and lively Inſtance of the little Progreſs we muſt ever hope to make in our Reſearches into the Nature of Things, without firſt ſtriking out what Light we can, by variety of proper Experiments.

16. The Urine that is made at the greateſt Diſtance from drinking, is obſerved to be

moſt rancid, as is in the like Caſe the Milk of Women; becauſe it has not only been longer a digeſting in the Blood, but alſo becauſe the more watry part goes off firſt in greateſt Proportion: And its being more eaſily retained in the Bladder, than the higher coloured Urine, ſhews that it is leſs rancid and ſtimulating, than a higher coloured Urine.

17. Whence it ſeems probable, that the Stone does not increaſe with an equable Progreſſion, in thoſe that are ſubject to it, but faſter or ſlower, in proportion to the more diuretic and diluted, or more rancid State of the Urine. Whence we may not unreaſonably conclude, that the Stone increaſes faſter in the Summer, when a greater Proportion of Moiſture is perſpired off, and the Urine rendered thereby more rancid, than in the Winter, when the Perſpiration being diminiſhed, a greater Quantity is diſcharged by Urine. Add to this that Heat is obſerved to contribute to the Induration of many Bodies. *Aretæus* on the contrary imagined the *Calculus* to be increaſed moſt in Winter and Autumn, *viz.* from a ſuppreſſed Perſpiration.

18. The *Lamina* or Cloves which are obſerved in many of the *Calculi*, are a further

Confirmation of this; for when the Urine does not depoſite its Tartar plentifully on the *Calculus*, then its Surface acquires, by rowling up and down in the Bladder, a kind of hard poliſh; but when the Urine returns again, to its more rancid tartarine State, then the *Calculus* is covered with a new Coat; the laſt poliſhed Surface, being the Partition between the two Cloves, at which Surface they more eaſily ſeparate than elſewhere.

EXPERIMENT IX.

1. I Put into a *Florence Flask* full of cold Water, a ſmall round rediſh Gravel Stone, about $\frac{1}{8}$th of an Inch Diameter, and alſo a Piece of a very hard *Calculus*, and having ſuſpended the Flask over a Fire, when the Water boiled, Air aroſe in plenty from the Gravel, whereby it was raiſed up in the Water, and much agitated to and fro; ſo that it looked like the *Nucleus* of a Comet, with its long Train of Air Bubbles ariſing from it.

2. When it had boiled for an Hour and half, on pouring in a little more hot Water, it ceaſed to boil for a Minute, and during that time, no Air aroſe from either of the *Calculi*.

3. An

3. An Hour and half after that, I poured into the Flask ſome more Water, which was much cooler, than what was poured in before: As ſoon as the Water boiled, I expected Air would ariſe again from the Gravel; but none roſe till it boil'd a conſiderable time: Then I took out the Gravel and found it waſted two thirds: But the very hard *Calculus* was not ſenſibly waſted, notwithſtanding ſome Air aroſe from that alſo in boiling.

4. I repeated the ſame Experiment, with two other large hard Gravel Stones, and a Piece of *Rheniſh Tartar*, which was diſſolv'd in a Quarter of an Hour, and found that when the Flask was filled up with very hot Water, then Air aroſe from the Gravel, ſoon after the Water began to boil again: But when what was waſted by boiling, was filled up with cold Water, then no Air aroſe from the Gravel, till it had boiled a long time.

5. Hence we ſee how the Alternacies of hot and cold, warm and cool, as they fix the Parts of the Gravel and thereby hinder their ariſing from it in the Form of Air, ſo they conduce much to the hardening of ſome Bodies. Thus the Parts of Animals and Vegetables do gradually coaleſce, and the *Calculus* hardens

more and more in the Bladder. And thus by Changes from very hot to ſudden cold, fatal Coagulations of the Blood are frequently made.

6. If there be any thing in the common Notion, that laying on one's back, contributes to the Increaſe of Gravel in the *Kidnies*, by heating of them, then this Experiment will ſhew one Cauſe which may contribute to it. Tho' I ſuſpect that a principal Cauſe of the firſt beginning of the Growth of Gravel in the Kidnies, is owing to the horizontal Poſture we are in when we lay in Bed: In which Poſture one of the *Kidnies* being lower than the *Bladder* when we lay on one Side, and both the *Kidnies* when we lay on our Back; the *Pelvis* or Cavity of the *Kidnies* becomes thereby the Sink for the tartarine Parts of the Urine to ſettle in. And the Urine being in that Poſture propelled by the *Kidnies* with a Force not only equal to the perpendicular Height of the *Bladder* above them; but alſo with a Force ſufficient to dilate the *Bladder* and circumambient Parts of the *Abdomen*; the Urine muſt needs preſs (eſpecially when the *Bladder* is in a good Degree full) with a conſiderable Force againſt the Orifices of the excretory Guts of the *Kidnies*; whereby the Progreſs

Progreſs of the Urine being in ſome degree retarded, it has more time to depoſite its *Tartar* in thoſe ſmall *Ducts* in the Papillæ, where it is thought the firſt minute Beginnings of Gravel are uſually formed; it being in Diſſections found there.

7. *Qu.* Might not theſe Inconveniences be in ſome meaſure obviated, by laying as Soldiers do in their *Barracks*, not in a horizontal but in a reclined Poſture, with the Head and upper parts of the Body conſiderably higher than the Feet and lower Parts?

8. In this Poſture the Urine flowing more eaſily down the *Ureters*, would not only more readily carry its tartarine Recrement along with it; but its Preſſure againſt the Orifices of the excretory *Ducts* being thereby taken off, it would more readily and in greater Plenty be ſeparated from the Blood: For which Reaſon it ſeems not improbable that an erect Poſture of the Body may contribute ſomething, among other Cauſes, that are with good Reaſon uſually aſſigned, for the greater Flow of Urine in the Day time, than in the Night.

9. The Care Nature has taken to prevent the Urine's regurgitating from the Bladder, by means of Valves placed at the lower Orifices

of the Ureters, where they difcharge themfelves into the Bladder, fhews the Importance of the Urine's not preffing on the Orifices of the excretory Ducts of the *Pelvis.* *Ruyfch* obferved the Kidnies of a Sheep extended to the Size of a Pint, occafioned by a Stoppage of Urine in the Bladder; and doubtlefs, leffer Degrees of Preffure on thofe *Ducts*, will proportionably incommode their Secretions.

10. And for the fame Reafons, it muft needs be advifeable, to take care to lay alternately fometimes on one Side, and fometimes on the other: For fince while we lay, fuppofe on the left Side, the excretory *Ducts* and *Sinus's* of the Kidney, will be in a low depending Pofture, for the Recrement of the Urine to fix in them; while at the fame time the excretory *Ducts* and *Sinus's* of the right Kidney will be in fuch a fuperior Pofture, as that the feparated Urine freely flows from them, and *vice verfa* when the right Kidney is lowermoft. Therefore it feems to be of no fmall Importance, to change Sides alternately, that whatever Sediment may have been fettled in the inferior Kidney, may by inverting it, be wafhed out, before it have time to fix into fmall Sand

and

and Gravel, whence it too eaſily increaſes into a larger Size.

11. And for the ſame Reaſon it ſeems adviſeable, that when we feel the firſt Beginnings of Uneaſineſs in one Kidney, we ſhould take care to have that be, for the moſt part, the uppermoſt Kidney, to try if haply by that Means, the firſt beginning Sand may be waſhed away.

12. It is to be ſuſpected that the firſt Beginnings of *Calculus* in Infants, may be frequently owing to their laying much upon their Backs, as well as to their long retaining of Urine in the Bladder while they ſleep much.

13. I am ſenſible that theſe may ſeem to ſome to be very trifling Precautions; yet for the abovementioned Reaſons I cannot think them ſuch; tho' I am far from imagining that they will be an effectual Preventive for all; yet as they may poſſibly be beneficial to ſome, it will ſure be worth the while to make ſo eaſy a Tryal, in a caſe of ſo great Importance to our Welfare.

14. *Helmont* ſays he was once afraid of laying on one Side conſtantly, leſt the Urine ſhould not paſs freely from the lower Kidney to the ſuperior Bladder, beſides what Impedi-

ment it might ſuffer from the Preſſure of the Bowels. But he was eaſed, he ſays, of this Fear, when he found two Perſons who lay conſtantly, the one on the right and the other on the left Side, and yet were neither of them troubled with *Calculi,* in thoſe lower Kidnies, but one of them in the upper. But notwithſtanding this and ſome other like Inſtances that I have known, yet it ſeems from the abovementioned Reaſons, moſt likely that the lower Kidney ſhould be moſt liable to breed Sand and Gravel.

15. Many obſerving one Kidney to be free from *Calculi* and the other not, concluded it was owing to the different Soundneſs of the Kidnies; ſome alledging that the excretory *Ducts* in the morbid Kidney were too narrow, others that it was too much relaxed; which they are often found to be: Which Relaxation may probably be owing to ſtopping the Secretion of Urine, and to a long Series of Pain from the *Calculus,* not that the Relaxation was the antecedent Cauſe, but the Effect of the *Calculus.*

Experiment X.

1. WHen we conſider the great Quantities of Air that are in our Food, whether animal or vegetable (without which it would be no Food) and at the ſame time reflect on the Diſpoſition that moſt of the Liquors which we drink have to depoſite tartarine Concretions, we cannot wonder to ſee the Urine of ſome Perſons ſo much diſpoſed to breed the *Calculus*: Which Quality of the Urine ſeems to ariſe in a greater Meaſure from our Drink than from our Food. Our Food, eſpecially that which is boiled, being more mucilaginous than the greateſt part of the Liquors which we drink, and conſequently leſs apt to depoſite its *Tartar*; of which Conjecture we have ſome further Confirmation from hence, *viz.* that by comparing the ſeveral Airs generated by Diſtillation, Fermentation, or Efferveſcence, from animal and vegetable Subſtances, with the Airs generated from their *Tartars*, which are Concretions moſtly from their fluid Parts, we find that the *Tartars* yield conſiderably more Air, than the ſolid Subſtances of Animals or Vegetables: Which

Which ſhews the Aptneſs of fluid Mediums, for tartarine Concretions to be formed in. And this holds true not only in Urine, and fermented Liquors, as Wine, *&c.* but the ſame may be obſerved in moſt Waters. Thus I find upon tryal that the Incruſtations of petrifying Springs are of a tartarine Nature; as are alſo the Incruſtations, which adhere to the Bottom and Sides of Veſſels in which Water has been frequently boiled.

2. For three hundred and eighteen Grains, or about half a cubic Inch of petrified Incruſtation from the *Cold Bath* in *Madingly* Wood near *Cambridge* being diſtilled in the Iron Retort, *Fig.* 38. *p.* 266. *Vol.* 1*ſt.* there aroſe from it three hundred and twenty ſix cubic Inches of Air, of which fifty four Cubes loſt its Elaſticity in ſix Days.

3. And from a hundred and ſix Grains of the ſame Subſtance mingled with Spirit of Salt, there aroſe in Fermentation ſeventy two Cubes of Air, all which in ſeven Days loſt its Elaſticity.

4. And it was the ſame with the Incruſtation of a Tea Kettle, in which Well Water had been boiled, which Well was dug below a *Stratum* of Gravel into a blue Clay: From

$\frac{3}{4}$ths of a cubic Inch of this Incruſtation I obtained by Diſtillation three hundred and twenty four cubic Inches of Air, of which one hundred and eighty loſt their Elaſticity in four Days.

5. And from a like Quantity of the much harder Incruſtation of the *New River* Water there aroſe in Diſtillation two hundred and thirty four Cubes, of which in four Days a hundred and eight loſt their Elaſticity.

6. The Air ariſes much ſlower from theſe Subſtances in Diſtillation, than from the *Tartar* of Wine or from the *Calculus Humanus*, for which reaſon a longer Continuance of an intenſe Degree of Heat is requiſite.

7. From three hundred and twenty eight Grains, or about half a cubic Inch of the powdered Incruſtation of boiled Well Water, let fall on two Cubes of Spirit of Salt, there aroſe eighty one cubic Inches of Air, all which loſt its Elaſticity in ſeven Days.

8. Three hundred and twenty eight Grains of the laſt mentioned Incruſtation, with two Cubes of Oil of Sulphur, generated two hundred and ſixteen Cubes of Air, all which did alſo loſe its Elaſticity in ſeven Days.

9. The

9. The ſame with the like Quantity of Oil of Vitriol generated a hundred and ninety eight Cubes, of which a hundred and twenty four were reſorbed in ſeven Days.

10. A hundred and forty ſix Grains of Incruſtation of a Tea Kettle from Water that came from Chalk at *Baſingſtoke* in *Hampſhire*, generated with Spirit of Salt a hundred and twenty ſix cubic Inches of Air, of which ſeventy two loſt their Elaſticity in ſeven Days. This Incruſtation was depoſited in ſuch Plenty from the Water, that in the Space of two Years the Cruſt was half an Inch thick.

11. Hence we ſee that theſe Incruſtations are of a tartarine Nature: They adhere alſo like *Tartar*, not only as Sediment to the bottom but alſo to the Sides of Veſſels: Whence we may not unreaſonably conclude, that many Waters which produce theſe, have Principles in them, which may promote the Growth of the Stone in the *Kidnies* and *Bladder*. For which Quality ſome Waters are more remarkable than others, as thoſe of *Paris*, which ſo fur up the Pipes, with theſe tartarine Incruſtations that the Water cannot paſs. And it is well known that the Inhabitants of *Paris* are more ſubject to the Stone

in

in the Bladder than thoſe of moſt other Cities. An Argument that Liquors contribute more to the breeding of the *Calculus* than our Food. Of which we have a further Proof in the Inſtance of ſmall Wines, which abound with *Tartar*, and which are too well known to ſubject thoſe who drink them to the Stone and Gout.

EXPERIMENT XI.

1. THAT Waters are impregnated with theſe tartarine Particles, more or leſs according to the different Nature of the ſeveral *Strata* or Beds of Minerals, Stone, *&c.* which they percolate thro', is evident from the following Experiments, *viz.*

2. A cubic Inch of *blue Clay* being diſtilled, a hundred and eight cubic Inches of Air were raiſed from it, of which thirty ſix Cubes loſt their Elaſticity. This Clay does not ferment with Spirit of Salt.

3. Three hundred and eighteen Grains of white *Italian Marble* being diſtilled, little Air aroſe, till it had a very great Heat, when two hundred and thirty four cubic Inches of Air were generated, of which fifty ſix Cubes loſt their Elaſticity in five Days.

4. And

4. And from the ſame Quantity of a tranſparent *Trapezium* like Spar, which came from the Mountains in *Switzerland*, two hundred and eighty eight Cubes of Air were raiſed, of which ninety Cubes loſt their Elaſticity in five Days. It was obſervable that the Air Bubbles which retained their Form for ſome time, did when their viſcid covering was burſt, emit Smoak, in the ſame manner as diſtilled Hart's-horn did in *Exper.* 77. *Vol.* 1*ſt. p.* 189.

5. From a like Quantity of *Selenites* there aroſe in Diſtillation, but thirty nine cubic Inches of Air, of which nine loſt their Elaſticity in five Days.

6. From one hundred and forty ſix Grains, or near ⅓d of a cubic Inch of *Chalk*, there aroſe in Efferveſcence, with two Cubes of Spirit of Salt, eighty one Cubes of Air, of which thirty ſix loſt its Elaſticity in nine Days.

7. From a like Quantity of a *Corraline Stone* and Spirit of Salt aroſe two hundred and eighty eight cubic Inches of Air, of which a hundred and ſixty two loſt their Elaſticity in ſeven Days.

8. From *Purbeck* Stone with Spirit of Salt a hundred and eighteen Cubes of Air, the greateſt Part of which loſt its Elaſticity in ſeven Days.

9. From

9. From *Fire Stone* and Spirit of Salt were generated a hundred and eight cubic Inches, of which thirty ſix loſt their Elaſticity in ſeven Days.

10. In like manner I found great Quantities of Air ariſe by Effervеſcence from many other foſſile mineral Bodies: As from *Portland* Stone, black Marble, blueiſh Marble, rediſh Marble, *Briſtol* Diamond, and the Lime-ſtone Marble in which it grows, alſo five different kinds of Spars, and a hard *Fluor*, two Stones that look'd like petrified Wood, and two others that reſembled petrefied Bones. But no Air aroſe from a hard rediſh paving Stone, which is brought in Ballaſt from *Norway*; nor from the *Darby*, *Collen* or *French* Mill Stones, nor from Iron Oar Stone.

11. Thus we find tartarine Principles in moſt kinds of foſſile Bodies; no wonder then that the Generality of Waters, which flow thro' them, are impregnated in ſome Meaſure with the like alkaline Qualities, ſo that thoſe mineral Waters which have wrongly been called *Acidulæ*, being found on further Examination to be alkaline, ſhould rather be called *Alkallulæ*. Tho' it is probable that moſt of the harder Bodies, ſuch as *Briſtol Marble*,

Spar and the like, communicate but little of theſe Qualities, in compariſon of what ſome ſofter kinds of Stone, Chalk, blue Clay or the like do.

12. Yet there are ſome Waters which depoſite no tartarine Incruſtation in the Veſſels they are boil'd in; ſuch is the Water which is convey'd in a Pipe, for the publick Uſe of the Inhabitants of *Hodſdon* in *Hertfordſhire*, which riſes from a Gravel, and boils up thro' a fine white Sand; which has no Incruſtation in a Boiler that has been uſed for fifteen Years: And ſuch is the Water with which Mr. Serjeant *Baynes*'s finely ſituated Houſe at *Havering* Hill in *Eſſex* is ſupplied with; the top of that Hill whence it flows, and on which the ancient royal Bower ſtood, being Gravel; whence as alſo from Sand the pureſt Spring Water has been obſerved to flow, if it have not firſt paſſed thro' *Strata* that it can diſſolve. Such alſo is the Water which his *Majeſty's Palace* at *Hampton Court* is ſupplied with, which leaves no Incruſtation in the Coffee-houſe Boiler, that has been in conſtant Uſe for fourteen Years: It has the ſame Quality at the Fountain Head at Mr. *Harvey*'s at *Comb:* And it is the ſame with the

Waters,

Waters, which ariſe at *North Homes* and in *Old Park*, which ſupply the *Dean* and *Prebendaries* and other Inhabitants of *Canterbury.* Theſe Waters come from gravelly Hills, and are conveyed thence in leaden Pipes, one from *Comb Hill* in *Surrey*, and the other from a like Hill about a Quarter of a Mile's Diſtance from *Canterbury.* So that Water ſeems not to contract any tartarine Quality from Gravel. And it is accordingly remarkable, that I cannot find upon tryal, any tartarine Quality in *Gravel* Stones or Flint, either by Fire or Fermentation.

13. *Hippocrates* condemns Water that is carried far in Pipes of Lead, yet three of theſe abovementioned Waters are carried a conſiderable way in leaden Pipes, that of *Hampton-Court*, about two Miles, and yet none of them have any incruſting Quality.

14. The *Comb* Water is obſerved to be ſofter, and to waſh Linen clean with a leſs Quantity of Soap than either *Thames* Water, or the Water of the River, which runs a-croſs *Hounſlow Heath* to *Hampton Court.* Whence it ſeems not improbable, that the Hardneſs of many Waters, and their curdling and coagulating of Soap may be in a good Meaſure

owing to the tartarine Quality with which they are impregnated.

15. The *Comb* Water is not filtrated thro' a great Depth of Earth before it comes to the Gravel, which is alſo the Caſe of the *Canterbury* Waters; and of thoſe at *Havering* Bower.

16. As *Comb-Hill* is Gravel in a manner to the Surface, and the Springs (many of which I have ſeen opened) ariſe at the Brow of the Hill out of that Gravel, the Water muſt partake greatly of the Nature of Rain Water; ſince the Dew and Rain which falls on that Hill, receives probably no other Alteration, in percolating thro' the Gravel only, than in coming out more pure and free from Sulphur than Rain Water is.

17. On comparing the Quantity of Sediment remaining after the Evaporation of equal Quantities, *viz.* thirty four cubic Inches of Rain Water and *Comb* Water, I found it exactly the ſame, *viz.* two Grains, which is in proportion to the Weight of Water as 1 to 4445: And the Sediment of *Havering Bower* Water was nearly in the ſame Proportion. The Method I made uſe of to do this with Accuracy was by cutting off the upper part of

Florence

Florence Flasks to a large Orifice, which being firſt weighed were filled with equal Quantities of Water which was alſo weighed, theſe being ſet on a Sand Furnace were there evaporated with a gradual equal Heat. The Sediment of the Rain Water was of a darker brown Colour than that of *Comb* Water; this laſt in a few Days melted ſo as to ſtand in ſmall Drops, whence 'tis likely there may be a very ſmall Degree of Salt in it, and that probably of a nitrous kind. From a like Quantity of *Scarborough Spaw* Water I procured forty eight Grains of Sediment, *i. e.* in the Proportion of one to a hundred and eighty five; it was near as white as double refined Loaf Sugar, in ſtanding ſome Days it melted and had a very nauſeous bitter Taſte, as that of *Ebſham* and the like purging Waters have. The Sediment from a like Quantity of *Briſtol* hot Well Water was four Grains, *i. e.* in the Proportion of 1 to 2222; it ſhot into *Stria* like the *Scarborough Spaw* Water, and was white like it; in ſtanding ſome Days it melted, but had no diſagreeable Taſte. On the Evaporation of half a Pound of *Havering* Well purging Water there remained $24 + \frac{1}{2}$ Grains of Sediment, *i. e.* as one to a hundred

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and forty three. From half a Pound of *Acton* Water there remained twenty two Grains of very white Salt, *i. e.* in the Proportion of 1 to 159.2. And from a like Quantity of *Ebſham* Water ſeventeen Grains, *i. e.* in the Proportion of 1 to 206.1.

18. It is obſerved of moſt Spring and Well Waters, that the fuller the Springs are, by great Falls of Rain, the ſofter they are: But when the Springs are at any time much fallen, for want of a Supply of Rain, then their Waters return to their former Hardneſs, which Quality we ſee they are impregnated with from the blue Clays, and other *Stratums* which they paſs thro'. Whence we ſee the Reaſon of the common Obſervation, *viz.* that the Water of Springs when they are very low, is not ſo good to brew with, as the Water of the ſame Springs when they are high and full.

19. Dr. *Mead* in his *Treatiſe of Poiſons* takes notice of a common Abuſe committed about *London*, *viz.* " in the chooſing of ſtagnating " impure *Well Water* for the brewing of *Beer*, " and making other Drinks. Such a Fluid " indeed has oftentimes a greater Force and " Aptneſs, to extract the Tincture out of " *Malt*, than is to be had in the more inno-

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" cent and ſoft Liquor of Rivers; but for this " very Reaſon it ought not, unleſs upon meer " Neceſſity, to be made uſe of; this Quality be- " ing owing to the *Mineral* Particles and *Alu-* " *minous* Salts, with which it is impregnated.

20. " A late *Author* (ſays the Dr.) viz. *Dr.* " *J. H. Scelera Aquarum, or a Supplement to* " *Mr.* Grant *on the Bills of Mortality*, by ſearch- " ing into the firſt Accounts of the Diſtem- " per we call the *Scurvey*, deſcribed by *Pliny* " *Lib.* 25. *c.* 3. and *Strabo Geogr. Lib.* 6. " under the promiſcuous Names of *Stomacace* " and *Scelotyrbe*; and examining the authentic " Hiſtories of it, in later Years, made by the " moſt obſerving Phyſicians in thoſe Countries " where it was unhappily revived, as *Olaus* " *Magnus*, *Balduinus Ronſeus*, *J. Wierus*, " *Solomon Albertus*, &c. finds that the Ori- " gine of it, was in all Times and Places " charged upon the Uſe of unwholſome ſtag- " nating *Waters*. Then by comparing toge- " ther the *Clayie Strata* of the Earth about " the Cities of *London*, *Paris* and *Amſter-* " *dam*, he ſhews that where the Water is " worſt, there this Malady is moſt rife. So " that he has put it out of all doubt, that " moſt of the perplexed and complicated

" Symptoms which are ranged under this one " general Name, if they do not entirely owe " their Birth to the Malignity of this Element, " do however acknowledge it to be their " main and principal Cauſe.

21. " And indeed *Hippocrates* himſelf, as " he has very plainly decypher'd this Diſeaſe " (*Prorrhet*, *l.* 2. *c.* 16.) by the Title of " σπλῆνες μέγαλοι, or *great Milts*; ſo he does " very particularly in another *Treatiſe* (*de Aere*, " *Aquis & Locis*, *ſub finem*) take notice, that " drinking of *ſtagnating Well Waters*, muſt " neceſſarily induce an ill Diſpoſition both of " the *Milt and Belly*.

22. " If we enquire into the Reaſon of ſuch " ill Effects, we muſt conſider that *Clay* is a " *Mineral Glebe*, and that the groſs Particles " and metallick Salts with which Waters paſ- " ſing thro' ſuch a bottom do abound, are, as " Dr. *Liſter* obſerves, not to be maſtered, that " they are indigeſtible in the human Body. " *De Fontib. Med. Angl. P.* 2. *pag.* 75. Not " only therefore will theſe cauſe, as he very " well argues, *calculous* Concretions in the " *Kidnies*, *Bladder* and *Joints*; and as *Hippo-* " *crates* experienced, hard Swellings in the " *Spleen*; but they muſt neceſſarily oftentimes

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"by their corroſive Quality twitch and irri-
"tate the ſenſible Membranes of the Stomach
"and Bowels, and thus hinder and interrupt
"the Digeſtion of our Food. Nay beſides
"all this, when they come into the Blood, it
"is no wonder if the ſmall Canals of inſenſi-
"ble Perſpiration are frequently ſtop'd and
"obſtructed by 'em; for it is upon this Score
"that *Sanctorius* teaches us, *heavy Water*
"*converts the Matter of Tranſpiration into an*
"Ichor, *which being retained, induces a* Ca-
"chexy. *Medicin. Static.* Sect. 2. Aphor. 6.

23. "What Miſchiefs will inſue hereupon
"every ones ſees; not only Pains in the Limbs,
"livid Spots in the Surface of the Body, Ul-
"cers, *&c.* from the Acrimony of the undiſ-
"charged Moiſture; but many beſides of thoſe
"perplexing Symptoms which go by the Name
"of *Hyſterical* and *Hypochondriacal* may take
"their Riſe from the ſame Source.

24. "If theſe Inconveniencies are often-
"times not felt, at leaſt not till towards the
"declining Age, in ſtrong and active Habits
"of Body; yet I am, from very good Expe-
"rience, aſſured, that they deſerve Conſide-
"ration in weaker Conſtitutions, and a ſe-
"dentary Life, eſpecially of the more tender
"Sex.

25. "I have the Honour to be nearly related "to a worthy Perſon, who led formerly an "afflicted Life from the frequent Returns of "violent *Colic Pains*, till ſhe was with happy Succeſs adviſed by the noble *Van Helmont* not to drink (as ſhe then did) Beer "brewed with *Well Water*; and her Health "is even now ſo far owing to this Management, that an Error in it is unavoidably "followed with the wonted Complaints.

26. "For theſe Reaſons *Pliny* tells us, *lib.* "31. *c.* 3. that *thoſe Waters are condemned* "*in the firſt Place, which when boiled do incruſtrate the Sides of the Veſſels*; and that "our *Well Waters* do this, no body who "looks into the *Tea-Kettles* of our Gentlewomen can be ignorant.

27. *Helmont* in his *Lithiaſis* mentions a petrifying Spring near *Bruxells*, which ſubjects the Monks to the Gripes, unleſs they daily uſe wild Carrot-ſeed boiled in Beer. There are indeed on the contrary ſeveral Inſtances of Perſons uſing petrifying and incruſting Waters without finding any the like Inconveniencies, or being ever ſubject to the Stone, but we cannot thence reaſonably infer that many pernicious Effects are not frequently produced by them.

28. To

28. To conclude, the foregoing Experiments, as they ſhew us the Nature of the Stone in the *Kidnies* and *Bladder*, and the Cauſes from whence they ariſe; ſo they may probably be of uſe, if not in leading us to the happy Diſcovery of a ſafe Diſſolvent, yet at leaſt they may ſerve to caution us to avoid ſuch things as are moſt likely to produce them, and direct us in the Uſe of ſuch Meats and Drinks, as will be moſt ſerviceable in preventing its Growth.

29. For tho' notwithſtanding the Urines of all Perſons have tartarine Matter in them, as is plain from the Adheſions to Urinals, *&c.* yet far the greateſt part of Mankind are never troubled with the Stone; and tho' moſt Perſons have Sand, yet not many have the Stone in the Kidnies, and fewer in the Bladder: It ſeems therefore not unreaſonable to think that if by proper Meat and Drink and other Precautions, the tartarine Quality of their Urine who are ſubject to the Stone could be ſomewhat abated; that then, they like others might be free from the firſt beginnings of it. But when once any Gravel is formed, it then too readily increaſes. For which reaſon it is a Matter of great Importance to uſe all proper

Means,

Means, to void it as ſoon as poſſible after it is fallen into the Bladder, before it be grown too big to paſs. I ſuſpect that a principal Reaſon why many Stones do not paſs off, is this, *viz.* the Stone by ſtimulating the Neck of the Bladder, cauſes a frequent Endeavour to make Water, when there is but little in the Bladder, whereby for want of a ſufficient Quantity of Urine to impell it forward it is retained: Whereas did the Patient contain his Water till the Bladder was well filled, there would not only be a greater impelling Force to puſh forward the Stone, but the Sphincter of the Bladder would alſo be thereby the more dilated, and ſo make more room for its Paſſage, eſpecially if the Urine were made mucilaginous by proper Drinks. And in caſe it ſhould not by this means paſs off, but cauſe a total Suppreſſion of Urine; it is well known that a Surgeon can eaſily remove it, by means of a Catheter; which Removal may poſſibly cauſe it to lay in a fitter Poſture to paſs off another time.

30. While I was intent upon theſe Experiments on the *Calculus*, it occurred to my Thoughts, that large Gravel Stones, which often ſtick for ſeveral Days in the *Urethra*, to the

the great Torment of the Patients; and which they cannot ſometimes be delivered from without cutting them out, might be drawn out by the following Inſtrument, *viz.*

31. I cut off the lower end of a ſtrait *Catheter* which made it a proper *Canula* for a Stillet or *Forceps* to paſs thro'; the lower end of the *Forceps* was divided into two Springs, like Tweezers, whoſe Ends were turned a little inwards; theſe Springs were made of ſuch a Degree of Tenderneſs, and Pliancy, as not to bear too hard againſt the Sides of the *Urethra*, by their Dilatation.

32. When this Inſtrument is uſed, the Springs are drawn up within the *Canula*; which being paſſed into the *Urethra* as far as to the Stone, the *Canula* muſt then be drawn back, ſo far as to give room for the *Forceps* to dilate; which dilated *Forceps* being then thruſt down a little further, ſo as to embrace the Stone, then the *Canula* muſt again be ſlid down, to make the *Forceps* take faſt hold of the Stone, ſo as to draw it out.

33. I ſent this Inſtrument to Mr. *Ranby*, to have his Opinion of it, who tells me that upon repeated Tryals he found it extracts theſe Stones with great Eaſe and Readineſs: And that

that it is ſo well approved of by other Surgeons, that many of them make uſe of it.

34. This ſtrait Inſtrument will therefore ſerve to extract ſuch Stones as are lodged, after they have paſſed the Turning at the *Os Pubis*; and I am informed that they are apteſt to lodge in thoſe Parts of the *Urethra* which are within the Reach of this ſtrait Inſtrument. But if it ſhould lodge a little beyond the Turn at the *Os Pubis*, it might probably be practicable to extract them thence by bending this Inſtrument, as the common *Catheters* are bent; if the Stillet were Silver it would bend the more eaſily.

35. Mr. *Ranby* is of opinion that this Inſtrument may be farther uſeful, in caſe of a Stricture or Contraction of any part of the *Urethra*, *viz.* by thruſting the *Forceps* into that Stricture, where by continuing ſome time, the conſtant Tendency of the Springs to dilate will widen the Stricture.

THE

THE APPENDIX,

CONTAINING

OBSERVATIONS and EXPERIMENTS

Relating to ſeveral SUBJECTS in the firſt VOLUME.

THE

APPENDIX.

AS these Obſervations and Experiments were intended to be inſerted under their proper Heads in the ſecond Edition of the firſt Volume; ſo I have here placed them in the ſame Order, they having no Connection with each other.

Observation I.

1. HAving in *Experiment* XIX. *Vol.* I. *p.* 53. found the Quantity of Vapour evaporated from a Surface of Water, in nine Hours in a Winter's-day, to be $\frac{1}{21}$ part of an Inch in Depth; it gave me occaſion to reflect on a vulgar Error, *viz.* that it is a more unwholſome Situation to dwell on the ſouthern, than on the northern Side of a River or other Water, *viz.* becauſe it is concluded that the Sun does by its Heat draw the Vapours up towards itſelf. It is certain that watry Particles do by the Action of Warmth,

riſe in the Form of Vapours, perpendicularly upwards, in a ſtill Air. But if that Air be in a progreſſive State towards any Point whatſoever, the Vapours are then conſtantly carried along with it, in a more or leſs oblique Courſe, according to the different Celerity of the Stream of Air. Heat is ſo far from attracting watry Vapours towards itſelf, that it on the contrary repells them every way from itſelf.

2. What has probably led Men into this Miſtake, is the common Obſervation, that when a wet Cloth is held before a Fire, the Vapours which ariſe from that Cloth, move conſtantly towards the Chimney in which the Fire is: Which Tendency is not owing to the Attraction of the Fire, but to the Force of the Stream of freſh Air, which is continually flowing in, to ſupply the Place of the Air, which being rarified by the Heat of the Fire is inceſſantly aſcending up the Chimney.

3. As it is obſerved that we have here in *England* more Southerly and South Weſt Winds, than from the oppoſite Points, ſo the Vapours muſt on that Account be blown more to the Northern than to the Southern Sides of Rivers. But the different Degrees of Healthineſs of

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one or the other Side of Rivers, on account of the Vapours which arise from them is so very inconsiderable that it seems to be meer trifling to take any notice of it, nor should I have done it here, had not the contrary been a very prevailing Opinion. The principal Advantage that the North Side of a River has above the South Side, seems to be this, *viz.* that it should be something warmer, on account of the Reflection of the Sun Beams from the Surface of the Water.

Observation II.

1. IT was found in the same XIX *Experiment*, *Vol.* I. on making an Estimate of the Quantity of Dew and Rain which is expended in Vegetation and Evaporation, that much the greatest part of what falls sinks down into the Earth, and is sufficient in Quantity to account for the Origin of Fountains, without having Recourse for Supplies, to the great Abyss of Waters; which is also further confirmed from the following Observations, viz. *Count Marsilli* in his *Histoire de la Mere*, *p.* 13. observes that Rivers from the neighbouring Mountains of *Languedoc & Provence*,

vence, do diſcharge themſelves by Under-currents into the Sea, and that at great Depths under Water, particularly at *Port Miou*.

2. And I am credibly informed, that Springs which come from the Hills near *Folkſtone* in *Kent*, are in like manner ſeen to boil up, thro' the Sands at the bottom of the Sea. A Proof that the Sea Water does not aſcend to the Tops of Hills and Mountains, and there form Springs and Rivers, but that theſe on the contrary deſcend into the Sea.

3. If the Sea Water percolated up to the Tops of Hills, then the high Cliffs immediately adjoining to the Sea, ſhould be conſtantly dripping with this percolated Moiſture, whereas they are uſually very dry. Particularly in the Iſle of *Wight*, there runs a Ridge of Chalk Hills along its Southern Coaſt with a high ſteep Cliff immediately bordering on the Sea, which Cliff is conſtantly dry. But the Springs running as they are determined by the dipping of the *Strata* of thoſe Hills, break out on the Northern Side of them, at a conſiderable Diſtance from the Southern Sea, where they form many Rivulets which run into the Sea, on the Northern Coaſt of the Iſland. So that we find the Northern Side

of theſe Hills at a conſiderable Diſtance and Height from the Sea, very wet with conſtantly running Springs, whereas the Southern Cliff which adjoins to the Sea is continually dry, and that juſt above the immediate Beat of the Waves.

4. It is well known that when large Quantities of Rain fall, that its Waters ſoak down to great Depths in the Earth, and do there repleniſh the Springs; and if Sea Water does the quite contrary, it muſt be owing to ſome peculiar Property in that Water, which can cauſe it to ſoak upwards to the tops of Mountains which are not only at a very great Diſtance from the Sea, but alſo ſome Miles higher than its Surface at high Water Mark.

OBSERVATION III.

1. DR. *Deſaguliers*, in his Abſtract of the firſt Volume of this Book, obſerves, from the Influence the Sun is found to have in rarifying the Vapours at 100 Feet Depth under Ground in *Experiment* XX. *p.* 64. "That the Heat of the Sun ſhould rarify the "Moiſture of the Earth to this Degree or a "great deal more, to drive it into the Roots

" of Plants, is very probable from ſome Ob-
" ſervations that Mr. *Beighton*, F. R. S. and
" himſelf made upon the Engine to raiſe Wa-
" ter by Fire, whereby it appears that the
" Steam or Vapour of boiling Water is rarer
" than Water from which it was produced,
" above 13000 times, when its Elaſticity is
" equal to that of common Air, *Philoſ. Tranſ.*
" No. 398.

OBSERVATION IV.

1. FROM the Influence the Sun had on the ſixth Thermometer, at two Feet Depth under Ground, ſo as to raiſe the Spirit in it to thirty one Degrees in *Experiment* XX. *p.* 64. *Vol.* I. we may hence ſee the Reaſon why when Cellars whoſe Walls, or the Crowns of their Arches are immediately expoſed to the Sun without any Shelter, they will ſeldom keep Wine or other ſpirituous Liquors well, *viz.* becauſe they are thereby too much heated.

2. I have alſo obſerved from thoſe Thermometers which were fixed at ſeveral Depths under Ground; that when in *March* the Sun has ſhined bright all Day, it has had a conſiderable

derable Influence in warming the Earth to ſome Depth, notwithſtanding a brisk very cold Eaſterly Wind blew all that Day. And doubtleſs the Sun has then the like Influence on the inner parts of Trees, whereby it agitates and raiſes the Sap: As alſo on the inmoſt parts of our Bodies when expoſed to it, while at the ſame time the Surface of them is pinched with cold Winds. And if there be any Truth in the common Notion, that ſitting ſtill for a conſiderable time in the Sun ſhine in ſuch Weather, is apt to cauſe Agues, this may not improbably be effected by the Influence of two ſuch oppoſite Degrees of Cold and Heat acting on the Body at the ſame time, whereby the Blood near the Surface for want of brisker Motion from Action, will be thickened; which is thought to be the State of the Blood when an Ague Fit comes on. And that the Blood moves but ſlowly near the Surface of our Body, when we ſit for a conſiderable time in the Cold, whether within Doors or without, we may be aſſured of from common Obſervation, *viz.* that tho' while we are thus ſitting we may not be ſenſible of any conſiderable Degree of Cold, yet on our firſt moving, whereby the Motion of our Blood is

alſo accelerated, we are then immediately ſenſible of Chilneſs all over us; which is doubtleſs owing to the very cool Surface-Bloods then running in greater Plenty among the next adjoining inner Veſſels, which being much warmer are then ſenſibly affected, with the Senſation of the much colder Blood from the Surface of the Body.

Observation V.

1. FROM the Proof given in *Experiment* XL. *Vol.* I. *p.* 128. of a free lateral Communication of the Sap-veſſels in Trees, we may ſee the Reaſon why, if the Bark be taken, for an Inch Breadth quite round, off from a lateral Branch, it will often kill the Branch below it, tho' on the oppoſite Side, *viz.* becauſe the disbarked Branch being thereby deprived of a conſiderable part of its Nouriſhment, which it received from between the Bark and the Wood, it muſt neceſſarily draw more vigorouſly, a greater Quantity of Nouriſhment, thro' its Sap-veſſels in the Wood, than the oppoſite Branch, whereby that Branch being deprived of its due Nouriſhment, will periſh, provided the Size of the disbarked

Branch

Branch be not ſo ſmall as to deprive the oppoſite Branch but of little of its Nouriſhment.

2. I have ſeveral times ſeen inſtances of this in a large *Catharine Pear* Tree in my Garden, which having on ſeveral of its Arms a young thriving Shoot, grown to about two Inches Diameter, this Shoot attracted Nouriſhment ſo vigorouſly, as not only to deprive of due Nouriſhment and kill the directly ſtrait Branch above it, but alſo the lateral Branches within eighteen Inches below it, whether on the ſame or on the oppoſite Side.

3. And I ſuſpect, that the blaſting of here and there a Branch of a Tree, may ſometimes be owing to the ſame Cauſe, *viz.* the depriving them of Nouriſhment by the too ſtrong Attraction of the other Boughs. This may ſometimes alſo be owing to a Defect in the particular Root which leads more directly to a dead Branch, as well as ſometimes to blaſting Qualities in the outward Air, which may be often ſufficient to kill Branches that are weakened by theſe or other internal Cauſes, and yet at the ſame time not be able to hurt the more vigorous Branches of the ſame Tree.

4. And that ſome Degree of Defect in Nouriſhment greatly ſubjects Fruit Trees to

 blaſting,

blaſting, is evident from common Experience; for when Trees are planted in a poor, or otherwiſe unkindly Soil, they are ſo liable to blaſt, that they very rarely bear any Quantity of Fruit.

Observation VI.

1. WHereas in *Experiment* XLVI. *Vol.* I. *p.* 141. I have put it only as a Conjecture, that I believed it would be found upon Tryal, that in the early Spring, the Sap moves firſt in the lower parts of Trees between the Bark and Wood, and not in the upper Part, as it would do, if the Sap aſcended thro' the Wood Veſſels, and then deſcended between the Bark and the Wood. And upon Enquiry, I am informed from Workmen that have been long accuſtomed to bark oaken Timber, that in the beginning of the Seaſon, the Bark often runs well in the Stem, but not in the Branches: And *vice verſa* at the latter end of the Seaſon it ſhall run well in the Branches but not the Stem: And I am pretty well aſſured from my own Obſervations, that it is the ſame in the Vine; whereas the Bark of the Top Branches ought firſt to be moiſtened if the Sap deſcends by the Bark.

Obser-

Observation VII.

1. TO other Arguments which are alledged against the Circulation of the Sap in *Experiment* XLVI. *Vol.* I. this may be added, viz. *Count Marſilli* a curious Enquirer obſerves in his *Hiſtoire de la Mere, p.* 57. 107. 173, that Sea Plants which are all rootleſs, except the *Alga*, have no longitudinal capillary Sap Veſſels, thro' which rooted Plants draw Nouriſhment to every part from the Earth. But the whole Subſtance of theſe Sea Plants is compoſed of Veſicles, which Veſicles receive their Nouriſhment immediately from the ſurrounding Water, and conſequently there being no longitudinal Veſſels to convey Sap from one end of the Plants to the other, we may reaſonably conclude that there can be no Circulation of the Sap; whence we ſee that Circulation is not neceſſary in order to Vegetation.

Observation VIII.

1. TO the Inſtance of the imbibing Power of grafted Stocks, mentioned in *Experiment*

periment XLVI. *Vol.* I. *p.* 147. we may add that of the imbibing Power of the Shoots of Fig-trees: For if the unripe latter Fruit continue on the Trees all the Winter, they will frequently kill the Shoot they adhere to, as is plain by the Mortifications beginning at the Stem of the Fig, and thence ſpreading thro' the whole Shoot, whereas other Shoots which had no Figs on them, have at the ſame time ſurvived, as I have found by repeated Tryals on Fig-trees which ſtand expoſed without the Shelter of a Wall: For which reaſon it is adviſeable to pull off the latter Crop Figs before the Winter comes on, which will be a means to preſerve the Shoots in our ordinary Winter; but in very ſevere Winters, ſuch as that in the Year 1728, all the bearing Shoots will be killed, unleſs ſheltered in a warm Situation. Now it cannot be ſuppoſed that this Mortification was occaſioned by a Circulation of the Sap between the bearing Shoot and the dead putrid Fig; but ſhould rather ſeem to be effected by the Shoots imbibing noxious Matter from the Fig. I have obſerved the like Effect from dry rotten Quinces which have hung on the Tree all the Winter. And it is doubtleſs in the ſame manner that a Canker ſpreads its Infection; the

ill

ill Effects of which are frequently prevented by cutting out the cankered Part.

Observation IX.

1. I Have in many Inſtances ſhewn that Air freely enters the Veſſels of Trees, and that it is in a great Abundance wrought into their Subſtance. *Qu.* May not the Uſe of thoſe ſpiral Wreaths that are coiled round the Inſides of thoſe Veſſels which are ſuppoſed to be Air Veſſels, and which are manifeſtly to be ſeen in ſeveral Trees, as alſo in the Leaves of the Vine and of Scabious, may not theſe be deſigned by Nature to promote the quicker Aſcent of Air, by being in ſome Meaſure conformed to its elaſtick Contortions? For ſuch ſpiral Wreaths ſeem to be altogether uſeleſs, for promoting the Aſcent of any Liquor, as the Sap which aſcends moſt freely thro' innumerable other capillary Veſſels which have no ſuch ſpiral Coils in them. Not that we are to ſuppoſe the Air in its elaſtic State actually to touch, and thereby to be determined in the Courſe of theſe Spirals as any Liquor would be. But as the Rays of Light when they are reflected from a ſolid Body, are found

to be reflected without actually touching the reflecting Body in the Point of Reflection; so it is not unreasonable to suppose, that elastic Air may like Light be diverted from one Course, and so be determined to another by the solid Bodies it approaches without touching them, but rebounding like Light from those solid Bodies near the Point of Contact.

2. I observe that these Spirals are coiled in a Course opposite to the Course of the Sun, *viz.* from West to East.

3. I have several times observed, that when the Fumes of burning Brimstone have ascended among the Leaves of a Tree, those Leaves have very soon withered; and that, not by any scorching Heat of those Fumes, for the burning Brimstone was at too great a Distance to have that Effect; whence it seems probable that these Fumes fix and destroy the Elasticity of some active elastic Principles in the Leaves: And *Qu.* may not some Blasts be owing to the like Effect, of Volumes of sulphureous Vapours which may float in the Air?

EXPERIMENT I.

1. IN *Experiment* LXVI. *Vol.* I. *p.* 181. I have given an Account of the Method I uſed to try what Proportion of Air could be obtained from Brandy, common Water, Rain, *Holt*, *Briſtol* and *Piermont* Waters. I procured in like manner a good Quantity of Air from *Spaw* and *Tunbridge* Waters.

2. 'Tis obſerved that when theſe Waters are deprived of this ſubtile elaſtic Matter, which is impregnated with a ſulphureous vitriolic Spirit; they then loſe their mineral Virtue and Effects, and will neither tinge with Galls or Syrup of Violets; and are alſo thereby deprived of their good Efficacy to thoſe who drink them.

3. I found by the like Tryals on *Ebſham* and *Acton* Waters, that they had little more of this elaſtic Matter than common Waters; doubtleſs it is the ſame with *Scarborough*, *Stretham*, and the like purging Waters. The Airs of ſome of theſe mineral Waters, either loſt their Elaſticity or were reſorbed again into the Water in ſtanding two or three Days; but a great part of that of *Ebſham* and *Acton* continued

continued for ſome Weeks before the Bubble which they formed on the top of the Water wholly diſappeared.

4. But from near two Quarts of *Bath* Water, I could ſcarce obtain a Quantity of Air equal to the Size of half a Pea, which may be owing both to its Warmth which will cauſe the elaſtic Spirit to fly off, and alſo to its ſubtile Sulphur which may fix that Spirit.

5. I heated ſome of this Water in an inverted Veſſel, which was half full of common Air, to try if the Fumes would abſorb any of it, but when the Veſſel was cold, I found none of it abſorbed. Whence we ſee, that when *Bath* Water cures Flatulencies in the Stomach, it does not do it by abſorbing the flatulent Air generated in the Stomach, but ſeems rather to effect it, by preventing the ariſing of much Air from the Contents of the Stomach, by means of the ſubtile Sulphur contained in it, in the ſame manner as the Fumes of Brimſtone do effectually check the Ferment of ſpirituous Liquors. And ſince the ſtrongeſt ſulphureous Fumes, ſuch as thoſe which ariſe from burning Brimſtone, as alſo from the violent Ferment, which is made by Spirit of Nitre, poured on powdered vitriolic

Stones,

Stones, ſince theſe Fumes are not found to abſorb the half of any incloſed Quantity of Air, there is therefore little Reaſon to expect that any the moſt prevalent Medicine ſhould cure theſe Flatulencies, by abſorbing what Air is already generated. We may rather conclude that it is effected by preventing, by means of the ſulphureous Quality of the Medicine, the Air's riſing too freely from the digeſting Contents of the Stomach.

6. And perhaps its Effect on the Blood may be ſomewhat of the ſame Nature, *viz.* to ſtrengthen its Texture by its fine ſulphureous and chalybeate Particles, which are alſo of the ſulphureous kind; whence probably leſs flatulent Secretions will be made from it into the Stomach and Bowels.

7. And as Rain Water is known to be impregnated with a fine Sulphur, eſpecially in warm Weather, it ſhould therefore be better than other common Water for the Flatulent to uſe: And if Rain Water ſtands ſome time to ſettle, and be then drawn into another Veſſel, 'tis ſaid that it will continue ſweet and fit to drink for a long time.

8. As this Experiment on *Bath* Water, and the obvious Deductions from it, carry me no farther,

farther, ſo I do not pretend to trace what other Effects theſe kinds of Waters may have, by rectifying in other Reſpects the Blood and Spirits, or by ſtrengthening the too relaxed Fibres of the Stomach and other parts of the Body: For *cæteris paribus* more Air will ariſe from the digeſting Ailments in a relaxed State of the Stomach, than when in a healthy firm State; it more vigorouſly embraces its Contents, in the ſame manner as fermenting Liquors emit more Air in open Veſſels, than when cloſe confined.

EXPERIMENT II

1. A *Florence* Flask being filled with Ale, and its Noſe inverted into a ſmall Veſſel full of the ſame Liquor; when about two cubic Inches of Air were riſen out of the Ale to the top of the inverted Flask, I poured that Air up into another inverted Flask full of Water: In ten Hours the greateſt part of this Air had either loſt its Elaſticity or was in part abſorbed by the Water, of which little remained the next Day.

2. Hence we ſee that while Air is riſing from Ale and other fermenting Liquors, ſome

of that Air is returning at the ſame time from an elaſtic to a fixed State: And to find out whether it was ſo or no, it was neceſſary to ſeparate in this manner, a Proportion of this Air from the Ale, which by continually ſending up freſh Supplies of Air, made it difficult otherwiſe to diſtinguiſh whether it were ſo or no.

3. From this Experiment on Ale we ſee the Reaſon why many of thoſe fermenting Mixtures mentioned in *Vol.* I. *Chap.* VI. were found to be ſometimes in a generating and ſometimes in an abſorbing State, *viz.* becauſe ſometimes they generated more than they reſorbed, and then they appeared to be in a generating State; and ſometimes, eſpecially on a Change from a warmer to cooler State, they then appeared to be in a reſorbing State, not that their reſorbing Power was greater in the cooler State, but becauſe little freſh Air was then generated, and part of what was before generated, was continually loſing its Elaſticity.

4. By ſeveral other the like Experiments I found that all the Air of fermenting Malt Drink did not loſe its Elaſticity, nor was reſorbed by the Water, and that, whether I

uſed large or ſmall Quantities of this Air, with either Rain or common Water, freſh or ſalted, unboiled or boiled, to clear it of its latent Air.

5. Neither, as was obſerved in the foregoing Experiment, will all the latent Air which is by Heat raiſed out of ſeveral Waters, be reſorbed again, or loſe its Elaſticity till after ſeveral Weeks, as was the Caſe of the Airs from the *Ebſham* and *Acton* Waters. But as to the *Pyrmont*, *Spaw* and *Tunbridge* Waters, whence a greater Plenty of this elaſtic Matter is raiſed by Heat, it does not continue ſo permanently elaſtic; which may be the Reaſon why ſeveral of theſe mineral Waters are obſerved to grow vapid and loſe their medicinal Spirit in ſtanding ſome time, tho' well corked up in Bottles, and even tho' put into a *Florence* Flask which was hermetically ſealed, as I was told by the late Dr. *James Keill* who tried the Experiment on a mineral Water near *Northampton*: So that theſe Waters may be thus deprived of what is called their Spirit, not only by its flying away, as it will ſoon do in open Veſſels, or when heated; but alſo by its being reduced from an elaſtic ſpirituous to a fixed State in the Water.

6. We

6. We may hence reaſonably conclude that Waters and moſt other Fluids contain in them both elaſtic and unelaſtic Particles; the elaſtic Particles readily expanding into Bubbles of a viſible Size, when the Weight of the incumbent Atmoſphere is taken off by the Air Pump. But the Quantity of Air which ariſes by this means, or by Heat, is but ſmall in compariſon of the Bulk of Water. Yet Monſieur *Mariotte* thought he had raiſed by Heat, from a drop of Water, a Quantity of Air, equal to eight or ten times the Bulk of the Drop of Water. *Eſſay de la Nature de l'Air*, *p.* 111. The Air which he raiſed by Heat aroſe doubtleſs from the Oil which ſurrounded the Drop of Water, for Oil abounds with elaſtic Air as I found by *Experiment* LXII. *Vol.* 1*ſt*. *p.* 179. In my *Experiment* LXVI. *Vol.* 1*ſt*. *p.* 181. I gave the Water a ſcalding Heat, if the Heat had been greater the Water would have expanded, ſo as to have either blown up the inverted Veſſel, or to have been all expelled forcibly out of it, as would have been the Caſe with Monſieur *Mariotte*'s Drop of Water at the Bottom of his inverted Glaſs Thimble, not only full of Oil, but alſo ſurrounded with it; if he had given it a greater Heat than I

gave the Water in my inverted Veſſel.

7. That the Air which is raiſed and ſeparated from Fluids takes up a much greater Space, and is conſequently more expanded than while in the Fluid, I found by the following Experiment, *viz.* I cemented to the Mouth of a half Pint Viol, a Glaſs Tube of three Inches Length, and half an Inch Diameter in bore: Then filling the Viol full of ſmall Beer, I put it in a Glaſs Veſſel which was ten Inches deep, filling the Veſſel with Water; I then placed over the Mouth of the Viol a ſmall inverted Glaſs Tunnel, made of the Neck of a *Florence* Flask, its ſmall Orifice being ſtopped with a Cork, which Cork reſted on the Mouth of the Viol. Then by reclining the deep Glaſs Veſſel, all the Air bubbled out from under the Bottom of the Viol, and from under the inverted Tunnel. Then placing the whole under an Air Pump Receiver, I exhauſted gradually ſo much Air, as to cauſe Bubbles to riſe out of the Beer into the Glaſs Tunnel, ſufficient to occupy a Space nearly equal to a cubic Inch. Then letting in the ſmall Quantity of Air which had been exhauſted out of the Receiver, I found the Quantity of Air in the inverted Tunnel to be many times greater

er than the ſmall empty Space in the Tube which was fixed to the Viol, which was wanting to be filled up with Beer, and which Vacancy was in a great Meaſure occaſioned by the Waſte of Beer, which was made by the Froth of the Bubbles, which flowed over the Mouth of the Viol. Whence it is evident that this Air takes up much more Space than when interſperſed in the Liquor. And in *Lowthorp's Abridgment of the Philoſ. Tranſ. V.* 2. *p.* 219. it was found that on exhauſting the Air Bubbles out of Water, the Bulk of the Water was ſcarce ſenſibly diminiſhed. Yet it cannot hence be inferred that the Air was not in an elaſtic State while in theſe Liquors. That there is elaſtic Air in common Water is certain from hence, *viz.* becauſe when frozen to Ice, the Air Bubbles are then by the uniting of many of them together, become large and viſible; yet Cold as 'tis well known does not increaſe but conſtantly Decreaſe the Expanſiveneſs of elaſtic Air.

8. Some have attributed the Expanſion of Ice in freezing to the large Collections of theſe Air Bubbles, which are not to be ſeen when the Ice firſt freezes, but are obſerved daily to increaſe in Bulk more and more; yet the Air

in theſe Bubbles is not forcibly pent up in them, as I found by the following Experiment, *viz.* having placed a Piece of Ice under Water, I pierced Holes into ſeveral of the Bubbles, yet the Air did not ruſh out with any Force, which it would have done, if it had been in a ſtrongly expanſive State.

9. But tho' a good part of the Air, which thus ariſes from Fluids, ſeems to have exiſted in an elaſtic State in thoſe Fluids; yet the Air which ariſes from ſolid Bodies either by the Force of Fire, or of Effervеſcence, does not ſeem to ariſe only from the Interſtices of thoſe Bodies, but principally from the moſt fixed Parts of them. For ſince the Airs which are raiſed by the ſame acid Spirit from a great Variety of Subſtances have very different Degrees of Permanency, as it was found in *Experiment* X. *Numb.* 3, 4, 5, 6, *&c.* and in *Experiment* XI. *Numb.* 6, 7, 8, 9, 10. of *Experiments on Stones*; hence 'tis probable that theſe Airs do not ariſe from latent Interſtices of the diſſolved Stones, *&c.* but from the ſolid fixed Subſtance of them. And ſince the whole of ſome of theſe new generated Airs, does in a few Days loſe its Elaſticity, it ſhould ſeem hence probable that whatever Air ariſes from

the Spirit in the Effervescence is not permanently elastic, or else that in the Solution of some Stones, it is thrown off into a more permanently elastic State than from others.

10. And that this Air, which is thus generated from solid Bodies by Effervescence, is not meer latent interstice Air, seems further probable from hence, *viz.* that Tartar which abounds so much with Air when dissolved with Spirit of Nitre in *Experiment* I. *Numb.* 4. *of Experiments on Stones*, generated no Air, which shews that certain effervescent or fermenting Degrees of Vibration are requisite, in order to throw the Particles of dissolving Bodies into a permanently elastic State.

11. There are other Instances in Nature, where the same Particles are sometimes in an elastic, and at other times in a fixed State: Thus in the Experiments on Electricity, the same Particles of Tinsel, Down, *&c.* are when approached by a well rubbed glass Tube, sometimes in a repulsive, *i. e.* elastic State, and sometimes in an attractive and cohering State. We may observe the same also in Water, whose Particles in a hot explosive State are vastly elastic, but in a frozen State, strongly cohering and fixed; and why may not Air Particles have

the ſame Properties? For as all the Parts of this Syſtem are in a conſtant oſcillatory Motion, ſo all Matter ſeems to be agitated by a repulſive and attractive Force.

12. But it is obſervable, that permanent Air ariſes chiefly and in greateſt Plenty, from Bodies which are ſpecifically heavier than Water, whoſe Attraction in a fixed State, and Repulſion in an elaſtic State, being on that Account greater than that of the lighter watry Particles, they are therefore better adapted to be (with the ſulphureous Particles, which they moſt ſtrongly attract) the principal Band of Union of ſolid Bodies, than the lighter watry Particles: Tho' doubtleſs all the Particles of Matter whatever do in actual Contact cohere, yet ſince it is found by *Experiments* XLIX and LV. *Vol.* I. that the moſt ſolid Parts of Animals and Vegetables, yield a vaſtly greater Quantity of Air and leſs Water, than the more lax and fluid Parts, which abound with a watry Fluid; it ſeems therefore hence reaſonable to conclude, that their Solidity is principally owing, not to the watry, but to the Air and ſulphureous Particles.

13. And the ſame holds true, when we conſider theſe Particles in an elaſtic State, for the

the ſpecifically heavier Air Particles continue in a much more permanently elaſtic State, than the lighter watry Particles; which tho' probably, by reaſon of the much greater Number of them in equal Bulks, do make a vaſt Exploſion when heated to a certain Degree, yet on the Abatement of that Heat, they inſtantly loſe their Elaſticity.

14. It will puzzle the *Epicureans* to give a rational Account how from a Chaos, a meer Neceſſity of Nature, and the caſual Concourſe of Atoms, ſo conſiderable a Quantity of Matter, as we find plentifully interſperſed in animal, vegetable and mineral Bodies, ſhould be endued with this double Capacity, of changing *pro re nata* from a ſtrongly attracting fixed State, to a permanently and vaſtly elaſtic State, and *vice verſa*; this wonderful Property of it, which is ſo neceſſary for carrying on the conſtant regular Proceſſes of Nature, muſt needs be owing to the Direction of an allwiſe Being.

OBSERVATION X.

1. WHEN I would diſtill Tartar, or any the like Subſtances which abound with great Quantities of Air, I have found it an

an effectual Way to prevent the Burſting of Receivers and Retorts, to lute at the Juncture a glaſs Tube eight or ten Feet long, and half an Inch in Diameter, thro' which the Air which ariſes in Diſtillation paſſes; the Length of the Tube at the ſame time preventing, in a good Meaſure, the flying off of the fine volatile Parts. By means of this Precaution a Retort may alſo with Safety be filled fuller of ſuch windy Subſtances as will not boil over without any Danger of burſting the Glaſſes.

Observation XI.

1. WHEN in *Experiment* LXXIV. *Vol.* I. *p.* 184. half a cubic Inch of *Sal Tartar* was diſtilled from an animal *Calx*, there aroſe from it two hundred and twenty four times its Bulk of Air, and the remaining Scoria did not run *per deliquium*, an evident Proof that all the *Sal Tartar* was diſtilled over: Which ſhews that *Sal Tartar* conſiſts of volatile Salt, firmly combined with Air Particles by the Action of Fire. For in the Diſſolution of the Parts of a vegetable Subſtance by Fire, a conſiderable Quantity of volatile Salt flies off into the open Air; while at the ſame time,

time, ſome of it is reduced to a fixed State, by being ſtrongly united in the Operation to Particles which are capable of being thrown into permanently elaſtic Air. This is very frequently obſerved in the making of Charcoal; the Duſt which covers the great Pile of burning Charcoal, being covered with the white volatile Salt which ariſes from the Wood; while at the ſame time another part of the volatile Salt which is found in the Form of *Sal Tartar* in the Aſhes of the Charcoal, is reduced to ſo fixed a State, that it is very difficult to volatilize it, unleſs it be mixed as in this Experiment with a *Calx*.

EXPERIMENT III.

1. IN *Experiment* XCVI. *Vol.* I. *p.* 224. I obſerved, when I let freſh Air into the glaſs Veſſel, *ay Fig.* 34. *p.* 168. that the ſulphureous Vapours which aroſe from the Mixture of Spirit of Nitre and vitriolic *Walton* Mineral, did abſorb the freſh admitted Air ſo faſt as to make the Water riſe very viſibly in the inverted Glaſs *ay*; but I did not then purſue the Experiment any farther, which I have ſince done.

2. For

2. For if after the Effervefcence was over when the Air *a z* was grown clear, frefh Air was then let into the inverted Glafs *a y*, there would immediately arife a violent Agitation between the two Airs, and they would become, from tranfparent and clear, a redifh turbid Fume; during which Effervefcence a Quantity of Air, nearly equal to what frefh Air was let in, would be abforbed; and if after this Effervefcence was over, and the turbid Air was again become clear, frefh Air was let in, it would again grow turbid and abforb Air as before, and that after feveral repeated Admiffions of Air. But after each Readmiffion of frefh Air, the Quantity abforbed was lefs and lefs, till no more was abforbed. And it was the fame after ftanding feveral Weeks, provided in the mean time, too much frefh Air had not been admitted.

3. Antimony and Spirit of Nitre at firft abforbed a little Air, it had not much Effect the firft Day, but the next Morning I found it generating Air at a good rate, with redifh Fumes. I then lifted the inverted Glafs *a y* from off the Bolthead, and immediately immerfed its Mouth in Water, in the Veffel *x x*, upon which in an Hour's time, it abforbed four

Inches

Inches Depth of Air; a Quantity equal to one fourth of the whole Capacity of the Glaſs Veſſel *a y*, and it did the ſame a ſecond, third and fourth time; on the fifth Admiſſion of freſh Air, it abſorbed but $3+\frac{1}{4}$ Inches Depth, but on the ſixth Admiſſion no more was abſorbed, nor was the Air turbid.

4. The Bolt-head *b* with its contained effervescent Maſs, I placed under a freſh Glaſs, upon which it abſorbed of this freſh included Air, faſter than the effervescent Mixture generated Air, ſo that the Water roſe in the inverted Glaſs; but after ſome time, the Water changed to a ſubſiding State, an Argument that more Air was then generated than abſorbed.

5. This Air inſtantly extinguiſhes a Candle, as do moſt of the Airs which are tainted with the Fumes of theſe effervescent Mixtures.

6. Spirit of Nitre with an equal Quantity of Water poured on Filings of Steel, abſorbed in an Hour a good Quantity of Air: Three Hours after, when the Air in the Glaſs *a y* was clear, I let in a like Quantity of freſh Air, but this did not make the Air in the Glaſs turbid, nor was any of the freſh Air abſorbed. Yet when at another time I let the like Mix-

ture of Water and Spirit of Nitre ſtand ſix or ſeven Days, and then admitted freſh Air, the Air in the Glaſs *a y* became immediately reddiſh and very turbid, and abſorbed Air, as faſt as the abovementioned Airs of Spirit of Nitre or *Aqua Regia* and Antimony; but it would not do in any conſiderable Degree a ſecond time.

7. When freſh Air is let into the Receiver *a y*, whoſe included Air is impregnated with the Fumes ariſing from the Mixture of compound *Aqua Fortis* or Spirit of Nitre, and *Whitſtable Pyrites* mentioned in the following Experiment, then the Air in the Receiver turns very red and turbid, and much Air is abſorbed after ſeveral repeated Admiſſions.

8. When freſh Air is thus admitted into the Glaſſes full of ſulphureous tho' clear Air, a good part of the Particles of the freſh Air muſt needs be reduced, by the ſulphureous ones from an elaſtic to a fixed State, as in the Effervescences of other Liquors. Therefore the riſing of the Water in the glaſs Veſſel *a y*, *Fig.* 34. does not ſeem to be wholly owing to the rebating of the Air's Elaſticity in ſome Degree, but rather to the Reduction of it from an elaſtic to a fixed State; which is fur-

ther

ther probable from hence, *viz.* that the whole Quantity of Air admitted at ſeveral times, is equal or nearly equal to the Quantity of ſulphureous Air *a z*, ſo that both Airs are at the ſame time contained within the Space *a z*.

9. When Mercury is diſſolved in compound *Aqua Fortis*, the Vapour ariſing from that Solution will alſo abſorb freſh Air.

10. The ſeveral Airs tainted with the Fumes of Vinegar and Oyſter-ſhell, Oil of Vitriol and Oyſter-ſhell, Belemnites and Vinegar, did not abſorb Air: But the Air of Belemnites and Spirit of Nitre did abſorb ſome freſh admitted Air, as did the Airs diſtilled from Tartar and *Newcaſtle* Coal; but Air diſtilled from an Ox's Tooth did not abſorb any.

11. Since we have under this Experiment ſo many Inſtances of the brisk Agitation and Efferveſcence, which ariſes from the Mixture of freſh Air, with Air which is ſtrongly impregnated with ſulphureous Fumes, which are raiſed by Effervescence from ſeveral mineral Subſtances; and ſince we have alſo from other Experiments many Proofs of the brisk Action and Reaction, there is between elaſtic Air and ſulphureous Particles, may we not with good Reaſon conclude, that the irkſome Heat

Heat which we feel in what is called a cloſe ſultry Temperature of Air, is occaſioned by the inteſtine Motion, between the Air and the ſulphureous mineral Vapours which are exhaled from the Earth? Which Efferveſcence ceaſes as ſoon as theſe Vapours are equably and uniformly mixed with the Air, as happens alſo in the Efferveſcences of other Liquors: Thus it is found that all other Fluids and even Metals in Fuſion do equally mix and diffuſe their ſeveral component Parts. The common Obſervation therefore that Lightening cools the Air ſeems to be founded on good Reaſon, that being the utmoſt and laſt Effort of this Effervescence.

12. May we not alſo with ſome Probability of Reaſon hence conjecture, that the firſt ignition or kindling of Lightening is effected by the ſudden Mixture of the pure ſerene Air above the Clouds, with the ſulphureous Vapours which are ſometimes raiſed in Plenty, immediately below them; the moſt dreadful Thunders being uſually when the Air is very black with Clouds, it rarely thundering without Clouds? The Clouds ſerving in this Caſe, like the inverted Glaſs *a z*, *Fig.* 34. as a Partition between the pure and ſulphureous Airs, which muſt therefore, upon their ſudden Admixture,

mixture, thro' the Interſtices of the Clouds, make like the two Airs in the Glaſs, a more violent Effervеſcence, than if thoſe Airs had without the Intervention of Clouds more gradually intermixed, by the conſtant regular Aſcent of the warmer ſulphureous Vapours from the Earth, and Deſcent of the cold ſerene Air from above. And tho' there was no luminous Flaſh of Light in the Glaſſes, yet when ſuch ſudden Effervеſcence ariſes, among a vaſt Quantity of ſuch Vapours, in the open Expanſe of Air, it may not improbably acquire ſo rapid a Velocity, as to kindle the ſulphureous Vapours, and thereby become luminous.

13. And ſince from the Effects that Lightening is obſerved to have on the Lungs of Animals which it kills, by deſtroying the Air's Elaſticity in them, as alſo from its burſting Windows outwards, by deſtroying the Air's Elaſticity on the Outſide of thoſe Windows; ſince it is hence probable that its ſulphureous Fumes do deſtroy a great Quantity of the Air's Elaſticity wherever it paſſes; it ſhould therefore cauſe the greateſt Commotions and Concuſſions in the Air, when the Air ruſhes into thoſe thus evacuated Places: Which it muſt neceſſarily do with great Velocity. Dr. *Papin* has

calculated the Velocity with which Air rushes into an exhausted Receiver, when driven by the whole Pressure of the Atmosphere, to be at the rate of one thousand three hundred and five Feet in a Second of time, *Lowthorp's Abridgment*, *Philos. Trans. Vol.* I. *p.* 586. A Velocity something greater than that of Sounds, which is according to Mr. *Derham* at the rate of twelve hundred and eighty Feet in a second, or a Mile in $4\frac{1}{8}$th Seconds; no wonder then that such violent Commotions should produce Whirlwinds, Hurricanes and Thunder-Showers, especially in the warmer Climates, where both the sulphureous and watry Vapours, being raised much higher and in greater Plenty, cause more violent Effects. These Thunderings are also observed to be frequent at the End of long Frosts, especially far North, and after long Drought, the sulphureous Vapours then arising plentifully out of the Earth.

14. If the first enkindling of Lightening were from burning *Focus's* of the Sun's Rays only, then there could be no Thunder Storms in the Night time, when the Sun was set, but only in the Day time, when it is above the Horizon; yet it is well known that Lightening

and

and Thunder are frequent in the Night, which ſhould therefore at that time ariſe from an Effer-veſcence in the Air; which Effervescence may very probably be frequently kindled into Light-ening, as the learned Dr. *Boerhaave* obſerves in his excellent and elaborate *Elementa Chemiæ*, *Vol.* I. *p.* 232. by the Refractions and Reflections of the Sun Beams among the Clouds.

15. That the crooked Dartings of Light-ening are not owing to its following a Train of ſulphureous Vapours in the Air, is probable hence, *viz.* that if a large Viol, which has all its Air exhauſted, be ſtruck on its Side with the Palm of the Hand, there will iſſue thence pale, livid, pointed and crooked Flaſhes, four, five or ſix Inches long, which Crookedneſs cannot in this Caſe be attributed to a Train of ſulphureous Vapours; for the ſame Event will happen, let the Viol be ſtruck never ſo often, whether it continues in the ſame Place or be removed. The *Impetus* therefore of Lightening ſeems to be given it, at or near the firſt Exploſion of the ſulphureous Vapours, its Force being more or leſs in Proportion to the Quantity of thoſe Vapours which are at once enkindled.

EXPERIMENT IV.

1. IT was found in *Experiment* XCVI. *Vol.* I. *p.* 224. that the *Walton Pyrites*, which is a vitriolic Stone, abſorbed more Air than it generated, with compound *Aqua-fortis* alone, and generated more than it abſorbed, when diluted with an equal Quantity of Water. But it is not the ſame with the vitriolic *Pyrites* which is gathered on the Sea Shore near *Whitſtable* in *Kent*, of which Stones green Vitriol or Coperas are there made; for when on five hundred and twenty five Grains or half a cubic Inch of it powdered, a cubic Inch of compound *Aqua-fortis* was poured, it expanded with a red Fume into a Space equal to two hundred and ſixteen cubic Inches, but in two Hours time that Space was wholly contracted, and a hundred and eight cubic Inches of Air were abſorbed. And the Fumes of this *Pyrites* were of ſo ſtrongly abſorbing a Quality, that when the *Aqua-fortis* was diluted with thrice its Quantity of Water, its Fumes abſorbed a hundred and forty four cubic Inches of Air more than was generated.

2. That theſe Fumes do at the ſame time both generate and abſorb Air, is further evident from the following Experiment, *viz.* I placed at once under a large inverted chymical Receiver, whoſe Orifice was immerſed in Water, five deep and wide glaſs Tubes which were cloſed at their lower ends, and bound faſt together, and ſupported at a due Height by a Stick in the midſt of them. There was in each of the Tubes a cubic Inch of compound *Aqua-fortis*, on which I let fall at two Hours Diſtance from each other, the powdered *Whitſtable Pyrites*. Upon the two firſt Mixtures more Air was abſorbed than generated; but the Effervefcences of the three following Mixtures generated ſeverally more than was abſorbed, *viz.* becauſe as appears in ſeveral other Inſtances, when the Air in the inverted Receiver was much impregnated with the ſulphureous Fumes of the two firſt effervefcent Mixtures, the Fumes of the three following Mixtures abſorbing no more Air, the Quantity generated by thoſe Mixtures, was ſeen by the ſubſiding of the Water in the inverted Receiver.

3. This Coperas Stone with Oil of Vitriol or Oil of Sulphur, or Spirit of Salt, each

diluted with Water, caufed a great Heat, but no vifible Effervefcence or Fume.

4. In thefe and the like Experiments I frequently made ufe of an inverted chymical Receiver inftead of the cylindrical Glafs, *Fig.* 34. which Receiver was fufpended by a Cord, which was braced round it: I firft poured the acid Spirit into the Bolthead or large Tube, and then put into the Top of the Bolthead an inverted Neck of a *Florence* Flask which formed a little Tunnel, whofe lower Orifice being flightly ftopped with a Cork or Plug of Cotten or Flax, I filled the Neck with the Powder, and at the fame time put into it a ftrong Piece of Wire, which was two or three Inches longer than the Neck; then placing the Bolthead or large Tube under the inverted Receiver, I drew up the Water into the Receiver to a proper Height by means of a Syphon: Then with my Hand I raifed up the Bolthead or large Tube, fo as to make the Wire bear hard againft the Top of the inverted Receiver, whereby the Cork being pufhed out of the Neck of the Flask, the Powder fell down into the Spirit.

5. But when the five Tubes abovementioned were placed at the fame time under the

Receiver, then I tied long Pieces of Packthread to the Top of each Wire, whereby pulling only one of thoſe at a time, the Powder fell then only into one Tube. I find theſe very ready ways to make theſe Experiments.

EXPERIMENT V.

AS this wonderful and important Fluid the Air is found to be very operative in every part of Nature, whether animal, vegetable or mineral, ſo we cannot eaſily find a larger Field, for making Variety of Experiments, or wherein we may reaſonably hope to have our Reſearches rewarded with further and more important Diſcoveries; particularly in relation to the great Uſe of it, to the Life and Support both of Animals and Vegetables, when pure and uncontaminated, without which it is ſo far from being beneficial, that it is noxious to them.

I will here therefore ſubjoin ſome Experiments which Mr. Profeſſor *Muſkhenbroek* made, with Variety of efferveſcent Mixtures both in the open Air and in *Vacuo*; an Account of which he has given in his *Additamenta ad Tentamina Experimentorum natura-*

lium captorum in Academia del Cimento.

" 1. A Mixture of three Drams of well
" rectified Spirit of Wine, and of an equal
" Quantity of Wine Vinegar, made no visi-
" ble Commotion, but acquired a Heat from
" forty four to fifty two Degrees on *Fahren-*
" *heyt's Thermometer.*

" 2. The ſame Mixture in *Vacuo*, made an
" inſtantaneous Ebullition, which ſoon ceaſ-
" ed, and acquired a Heat from forty four to
" forty nine Degrees. The Mercury in the
" Air Pump Gage deſcended two Lines on
" account of the elaſtic Matter generated in
" the Receiver, the Capacity of which was
" equal to a hundred and forty two *Rynland*
" cubic Inches.

" 3. Half an Ounce of Spirit of Wine,
" and a Dram of Spirit of Sea Salt did, when
" mixed, without any ſenſible Commotion,
" acquire a Heat from forty ſix to fifty.

" 4. The ſame when mixed in *Vacuo* ac-
" quired one Degree of Heat more.

" 5. *Monſieur Geoffroy in l'Hiſt. de l'Acad.*
" *Roy. Anno* 1727. ſays, that almoſt all the
" eſſential Oils of Plants which are prepared
" by Diſtillation, do when mixed with recti-
" fy'd Spirit of Wine produce ſome Degree of
" Cold,

"Cold, which Mr. *Muskhenbroek* found to be "greater when they were mixed in *Vacuo*; "in the Mixture of Spirit of Wine and Oil "of Fennel, the Degree of Cold was in *Vacuo* "from forty four to forty two Degrees, which "in the open Air was not ſenſible.

"6. When Oil of Caraway and Spirit of "Wine were mixed in *Vacuo*, the Degree of "Cold was from 45 $\frac{1}{2}$ to 41 $\frac{1}{2}$, which Cold "was but half a Degree in the open Air.

"7. Half an Ounce of Oil of Turpen-"tine, and a like Quantity of Spirit of Wine, "did on Mixture acquire Degrees of Cold "from forty five to forty three Degrees, and "a like Mixture in *Vacuo* to forty two De-"grees.

"8. On a Mixture of half an Ounce of "Vinegar with a Dram of red coral, there "was a great Effervеſcence like the boiling "of hot Water, with many Air Bubbles, the "Heat was from forty four to forty ſix $\frac{1}{2}$ "Degrees.

"9. When a Dram of Crabs Eyes was "put into half an Ounce of Vinegar, there "was inſtantly a great and laſting Efferveſ-"cence, with much Froth; the Heat was in-"creaſed from forty four to forty ſix Degrees.

"10. On

"10. On mixing the like Ingredients in "*Vacuo*, there was a very great Effervefcence "with much vifcid Froth; but what was ve-"ry remarkable the Thermometer fell from "forty four to forty three Degrees; and the "Mercury in the Air Pump Gage fubfided "four Lines, the Stones were much lefs dif-"folved than in the foregoing ninth Experi-"ment.

"11. There was a vifible Effervefcence tho' "little Froth, when half an Ounce of Vi-"negar and a Dram of white Chalk were "mixed, the Heat was increafed from forty "four to forty five $\frac{1}{4}$th.

"12. When the fame Experiment was re-"peated in *Vacuo*, there was a great frothing "Effervefcence, but the Thermometer de-"fcended from forty four to forty three: "And there was fo much elaftic Fluid gene-"rated, as made the Mercury in the Gage "fall four Lines.

"13. And the Effects were nearly the fame, "both in the Air and in *Vacuo*, where Vi-"negar and a blue *Namur* Stone were mixed.

"14. Three Drams of Spirit of Sea Salt "being poured on a Dram of Filings of Iron, "there arofe a fmall tho' fenfible Effervefcence, whereby

" whereby the Heat was increaſed from for-
" ty ſeven to fifty ſeven Degrees.

" 15. But when the like Mixture was made
" in *Vacuo*, there was a great and frothy Ef-
" ferveſcence, which continued long, and
" diſſolved the Metal much more than in the
" foregoing Experiment; the Heat increaſed
" from forty ſeven to ſeventy Degrees; the
" Mercury in the Gage did not ſtir.

" 16. A Dram of Biſmuth being thrown
" into a like Quantity of Spirit of Sea Salt,
" there aroſe a vaſt frothy Efferveſcence, with
" white Fumes; the Heat was ſo great as to
" riſe from forty ſeven to a hundred and fif-
" teen Degrees. The ſame being repeated in
" *Vacuo*, there was a very great frothy Effer-
" veſcence, with riſing Fumes, but the Heat
" was only from forty ſeven to ninety four De-
" grees; the Mercury in the Gage fell four
" Inches.

" 17. A Dram of Golden Marchaſite be-
" ing thrown into three Drams of Spirit of
" Salt, there was no Efferveſcence, and very
" little of it diſſolved in a Month, the Heat
" only from forty ſeven to forty eight $\frac{1}{4}$ De-
" grees.

" 18. But

"18. But in *Vacuo* there was a remarkable "frothy cold Effervefcence, for the Thermometer funk one Degree: The Mercury in "the Gage did not move, more of the Marchafite was diffolved than in the former Experiment.

"19. Three Drams of this Spirit of Salt "with a Dram of red Coral, made a vehement frothy Effervefcence; its Heat was "from forty feven to fifty fix Degrees.

"20. The like Mixture in *Vacuo* made a "vaft frothing Effervefcence, with the fame "Degree of Heat as before: The Mercury in "the Gage fell $3\frac{1}{12}$th Inches.

"21. A like Quantity of Spirit of Sea "Salt, being poured on a Dram of pounded "Marble, there was a great frothy continued "Effervefcence, the Heat of which was from "forty feven to fifty feven Degrees.

"22. The like Mixture in *Vacuo* made a "vaft Effervefcence, which was foon over; "but its Heat was only from forty feven to "fifty two Degrees. The Mercury in the "Gage defcended $3\frac{1}{4}$ Inches, by reafon of the "elaftic Matter which was generated in the "Receiver.

"23. A

" 23. A Dram of Ox Bone being put into " three of Spirit of Salt, raiſed a great frothy " Effervefcence of ſome Continuance; the " Heat of it was from forty ſeven to fifty " ſeven.

" 24. The ſame being repeated in *Vacuo*, " made a greater Effervefcence, tho' not of " ſo long Duration; the Heat riſing only to " fifty five Degrees.

" 24. Spirit of Nitre with Rain Water " cauſes a Heat from forty five to fifty three " Degrees, with an equal Quantity of diſtill-" ed Elder Water from forty ſeven to fifty one " Degrees. The ſame Mixture in *Vacuo* cauſ-" ed a viſible Effervefcence, with ſome Vapour, " the Heat was from forty one to fifty five " Degrees.

" 25. Scurvy-graſs Water mixed with an " equal Quantity of Spirit of Nitre, cauſed " an immediate ſhort Perturbation, which " raiſed a Heat from forty ſix ½ to fifty five " Degrees.

" 26. The ſame being repeated in *Vacuo*, " there was a ſort of Effervefcence with Va-" pours, the Heat was alſo fifty five Degrees.

" 27. A Dram of *Ceruſe* with Spirit of " Nitre made a great and hot Effervefcence,

" *viz.*

" *viz.* from forty ſix to fifty eight Degrees.

" 28. But the ſame Mixture in *Vacuo* " made a conſiderable Ebullition with Froth, " its Heat was from forty ſix to ſeventy two " Degrees. The Mercury in the Gage did " not ſtir.

" 29. A Dram of *Saccharum Saturni* with " Spirit of Nitre, cauſed no ſenſible Commo- " tion, yet the Heat roſe from forty ſix to " fifty two Degrees.

" 30. But the ſame Mixture in *Vacuo*, made " a conſiderable ſhort Effervеſcence with Froth, " the Heat roſe from forty ſix to fifty four " Degrees.

31. A Dram of *Minium*, thrown into Spi- " rit of Nitre, made a ſmall, but viſible Effer- " veſcence, almoſt without Froth or Vapour.

" 32. But the like Mixture in *Vacuo* made " a remarkable Effervеſcence with Froth, of " long Continuance, and ten times greater " than before the Heat increaſing from forty " ſix to eighty eight Degrees.

" 33. A Dram of *Lytharge* in Spirit of Ni- " tre, made a conſiderable Effervеſcence with " Froth, which ſoon ceaſed; the Heat in- " creaſed from forty ſix ½ to ſixty two De- " grees.

" 34. The

" 34. The like Mixture in *Vacuo*, made
" an Effervefcence of long Continuance, its
" Heat increafing from forty fix ½ to fixty
" Degrees.

" 35. A Dram of Tin dropped into Spirit
" of Nitre, did in an inftant make a moft fur-
" prizingly violent Effervefcence, fo that the
" Heat increafed from forty fix ½ to two hun-
" dred and fifty; the Fumes were fo great as
" to fill the whole Houfe, and fume out at
" the Door: The whole Tin was inftantly
" turned into a white dry Powder, refembling
" true Calx of Tin. This Experiment ought
" to be made with great Caution, left the
" Lungs be injured thereby. When *Aqua-*
" *fortis* and Tin were mixed the Heat was
" only from forty fix to a hundred and fixty
" three Degrees.

" 36. When a Dram of Tin was dropped
" into Spirit of Nitre in *Vacuo*, there was
" alfo a violent Effervefcence, tho' not near fo
" great as the other, the Heat arofe from forty
" fix ½ to a hundred and eighty Degrees. Some
" of the volatile Fumes were elaftic, for the
" Mercury in the Gage fell two ⅓ Inches.

" 37. Filings of Iron and Spirit of Nitre
" made a vaft frothy Effervefcence, with large
" yellow

"yellow fetid Fumes, the Heat rofe from forty fix to a hundred and forty five Degrees.

"38. The like Mixture in *Vacuo*, boiled much, with thick yellow Fumes, its Heat was from forty fix to a hundred and twenty Degrees: The Mercury in the Gage defcended four $\frac{1}{2}$ Inches; with fuming Spirit of Nitre the Heat is fo fudden and great as to break the Thermometers.

"39. With a Dram of Filings of red Copper and Spirit of Nitre, the Effervefcence with yellow Fumes was great, the Heat from forty fix to a hundred and fix; a fmall part of the Copper being diffolved, it made a fine green Tincture.

"40. The like Mixture in *Vacuo* made a great Effervefcence, the Heat being from forty fix to a hundred Degrees: The Mercury in the Gage defcended three $\frac{1}{2}$ Inches.

"41. Spirit of Nitre and a Dram of Brafs made a vaft Effervefcence, with plentiful red hot Fumes, the Heat thence arofe from forty eight to a hundred and eighty Degrees. All the Metal was diffolved, affording a beautiful green Tincture.

"42. From the like Mixture in *Vacuo*, arofe a very great Effervefcence with plen-

"tiful

" tiful Fumes, the Heat increasing from forty
" eight to a hundred; all the Metal was like-
" wise presently dissolved into a Tincture like
" the former. So that the Effect of Spirit of
" Nitre is in *vacuo* the same on Brass and
" Copper, but very different in the open
" Air. The Mercury in the Gage subsided
" $3\frac{1}{12}$ Inches.

" 43. A Dram of Filings of Silver and
" Spirit of Nitre did effervesce, but not much,
" some Fume flew off; the Heat increased
" from forty eight to fifty seven.

" 44. The like Mixture in *vacuo* made an
" Ebullition, like boiling Water, but with
" little Froth; but, what is wonderful, there
" was no Heat excited the Thermometer
" at forty eight Degrees.

" 45. Spirit of Nitre poured on a Dram of
" Bismuth, made an inexpressibly violent Ef-
" fervescence, and such plentiful Fumes as
" filled the whole House like those from
" Tin: The Heat increased from forty eight
" to two hundred and forty three. When
" the Ebullition ceased the Mixture fell into
" a dry yellowish *Calx*.

" 46. The like Mixture in *vacuo*, made
" a vast Effervescence with plentiful Fumes,

" which like Dew ran down the Sides of " the Receiver; the Heat increaſed to a " hundred and fifty. The Mercury in the " Gage deſcended 2 $\frac{2}{3}$ Inches; leſs of this " Metal was reduced to a *Calx*, than in the foregoing Experiment.

" 47. A Dram of Golden *Marchaſite* " dropped into Spirit of Nitre, made a great " frothing Ebullition, with plenty of thick " yellow Fumes, it was almoſt all diſſolved.

" 48. A Dram of crude *Antimony*, with " Spirit of Nitre, made an Ebullition, like " that of boiling Water, with ſome Fumes, " the Heat roſe from forty ſix to ſeventy three " Degrees; the greateſt part of it was undiſ- " ſolved.

" 49. The like Mixture in *vacuo* made a " conſiderable Ebullition with Froth, and " plentiful Fumes; the Heat was alſo from " forty ſix to ſeventy three, leſs of this was " diſſolved than of that in the open Air. The " Mercury in the Gage deſcended 2 $\frac{1}{2}$ Inches.

" 50. A Dram of the *Lapis Calaminaris* " mixed with Spirit of Nitre, cauſed a viſi- " ble Emotion, and the Heat was from forty " ſix to ſixty Degrees.

" 51. The like Mixture in *vacuo* made a
" very remarkable Ebullition, with plentiful
" Fumes, which covered the Sides of the
" Receiver; the Heat being increaſed from
" forty ſix to a hundred and two Degrees.

" 52. A Dram of *Tutti* mixed with Spirit
" of Nitre, cauſed no viſible Motion, the
" Heat was increaſed from forty ſix to ſixty
" nine Degrees.

" 53. The like Mixture in *vacuo* made a
" remarkable frothy Effervеſcence, with a
" Heat from forty ſix to eighty Degrees;
" more of it was diſſolved than in the fore-
" going Experiment. The Mercury in the
" Gage deſcended two Lines.

" 54. A Lixivium of calcined Aſhes, with
" an equal Quantity of Spirit of Nitre, made
" a vehement Effervеſcence, with Froth and
" Plenty of Fumes, the Heat being increaſ-
" ed from 46 ½ to eighty five Degrees.

" 55. A like Mixture in *vacuo* made a
greater Effervеſcence, tho' its Heat was leſs,
" *viz.* from 46 ½ to ſeventy four Degrees.
" The Mercury in the Gage fell ſeven Inches.

" 56. Spirit of Nitre mixed with an equal
" Quantity of new Milk, made no ſenſible
" Emotion, but yet the Heat increaſed from
" 47 to 55 ² Degrees.

" 57. When Spirit of *Sal Ammoniac* and " Spirit of Nitre were three Drams of each " mixed, they made ſome Ebullition, and the " Heat roſe from forty ſeven to eighty three " Degrees; the Mixture continued limpid and " colourleſs.

" 58. When theſe two Liquors were plac- " ed in diſtinct Veſſels under an Air Pump " Receiver, they both fumed while the Air " was exhauſting and after it was all exhauſt- " ed. As ſoon as the Spirit of Nitre was " poured on the Spirit of *Sal Ammoniac*, " there roſe an inſtantaneous Motion, which " like exploding Gun-powder, diſperſed part " of the Liquor; but when theſe Spirits were " afterwards mixed more gradually, then the " following Exploſions were leſs violent, the " Heat increaſed from forty ſeven to ſixty " three Degrees. The Mercury in the Gage " deſcended four Inches.

" 59. Tho' when equal Quantities of freſh " Urine and Spirit of Nitre were mixed, " there was no ſenſible Efferveſcence, yet the ' Heat increaſed from forty ſeven to fifty two " Degrees.

" 60. The ſame Mixture in *vacuo* had no " viſible Commotion, yet the Heat was raiſed " from

" from forty ſeven to fifty ſeven Degrees.

" 61. Spirit of Nitre, mixed with an equal " Quantity of Spirit of Vinegar, there was " ſcarce any viſible Commotion, yet the Heat " aroſe from forty ſix to fifty four Degrees.

" 62. The ſame Mixture in *vacuo* had a " ſmall Commotion during its Mixture, and " acquired a Heat from forty ſix to fifty ſix " Degrees. The Mercury in the Gage did " not ſtir.

" 63. Spirit of Nitre, with half a Dram of " Crabs Eyes mixed, made a conſiderable " frothing Efferveſcence, whereby the Heat " was increaſed from forty ſix to fifty four " Degrees.

" 64. The ſame Mixture did in *vacuo* make " a great frothing Effervefcence, four times " greater than the former, its Heat was raiſ- " ed from forty ſix to fifty ſix Degrees, they " were wholly diſſolved in both Caſes.

" 65. Equal Quantities of Lemon-juice " and Spirit of Nitre, cauſed no viſible Emo- " tion when mixed, and tho' the Spirit of " Nitre ſunk inſtantly to the Bottom, the " Lemon-juice ſwimming on the top, yet the " Heat increaſed from forty ſix to 52 ½ De- " grees.

 " 66. The

" 66. The ſame Mixture in *vacuo*, tho' it
" cauſed no viſible Emotion, yet raiſed a Heat
" from forty ſix to fifty ſix Degrees, the Mer-
" cury in the Gage ſtanding ſtill.

" 67. Equal Quantities of Spirit of Nitre
" and *French* White Wine, when mixed,
" cauſed a Heat from forty ſix to fifty three
" Degrees, yet there was no viſible Emotion
" in it.

" 68. Equal Quantities of Spirit of Nitre
" and diſtilled Oil of Saſſafras Wood, made
" a violent Efferveſcence, with Fume and
" Heat. Yet Spirit of Nitre and two Drams
" of Oil of Anniſeed, when mixed, neither
" made any Emotion, nor Alteration in the
" Heat.

" 69. *N. B.* This Spirit of Nitre was made
" with Bole, it emits few Air Bubbles in *va-*
" *cuo*; whereas the fuming Spirit of Nitre,
" and Spirit of Sea Salt abound with Plenty
" of Air; hence before theſe are mixed in
" *vacuo*, it is requiſite to wait for ſome time
" till they are freed from their Air, that we
" be not deceived by taking Air Bubbles for
" Efferveſcences.

" 70. *Monſieur Geoffroy*'s fuming Spirit of
" Nitre, when mixed with Oil of Turpentine

" or

" or other eſſential Oils of Plants, does in-
" ſtantly kindle into a great Flame; this Spi-
" rit is made by diſtilling in a reverberatory
" Heat two Pounds of Nitre with one of Oil
" of Vitriol.

" 71. Twenty Drops of this Spirit being
" mixed in *vacuo* with a like Quantity of
" Oil of Carraways; they made a great Ef-
" ferveſcence without Flame, and the Heat
" was raiſed to two hundred and ſixteen De-
" grees: When the inteſtine Motion ſeemed
" to be over, he let Air into the Receiver, on
" which there was a Flame raiſed, which
" was ſoon extinguiſhed, by the great Fumes,
" for want of a free Air. Neither did Oil
" of Turpentine, Oil of Roſemary, or Oil of
" Anniſeed flame in *vacuo*, nor when Air was
" let in, till a little Oil of Vitriol was added,
" which yet did not cauſe the Oil of Anniſeed
" to flame.

" 72. Three Drams of Oil of Vitriol being
" mixed with an equal Quantity of Rain
" Water, there was no viſible Emotion, yet
" the Heat increaſed from forty eight to nine-
" ty two Degrees.

" 73. Three Drams of Oil of Vitriol, and
" a like Quantity of the diſtilled Water of

"Scurvy-graſs, being mixed, made no Emo-
"tion, but the Heat was greater than with
"Rain Water, *viz.* from forty eight to nine-
"ty two Degrees.

"74. Like Quantities of Oil of Vitriol
"and diſtilled Elder Water cauſed on Mix-
"ture a Heat from forty eight to ſeventy
"Degrees. Whence he obſerves that Elder
"Water, which when drank refrigerates the
"Body, has ſome Property in it, which
"checks Heat to twenty two Degrees leſs
"than Rain Water; whereas Scurvy-graſs
"Water which heats the Body, has ſome-
"thing in it which with Oil of Vitriol cauſes
"by ſix Degrees a greater Heat than Rain
"Water.

"75. Three Drams of Oil of Vitriol and
"a like Quantity of Rheniſh Wine, when
"mixed, cauſed a Heat from fifty nine to
"99 ½ Degrees. He further obſerved that
"when the Proportion of Wine in the Mix-
"ture was either greater or leſs, that the
"Heat was in both Caſes leſs.

"76. Two Drams of *Sal Ammoniac* be-
"ing thrown into three Drams of Oil of Vi-
"triol, there was inſtantly a great frothing
"Effervescence, which filled the whole Place

"with

"with acrid Fumes: Which Fumes were ſo "warm that they raiſed the Thermometer, "which hung over them, ten Degrees; "while at the ſame time another Thermo-"meter which was placed in the Mixture, "deſcended by reaſon of the increaſed Cold "from ſixty to forty eight Degrees; the "greateſt part of the Salt was diſſolved. If "while the Effervescence continues, ſome "Water be poured in, the Thermometer, "which was in a deſcending State on ac-"count of the increaſing Cold, will imme-"diately aſcend on account of the then much "increaſed Heat.

"77. He repeated this remarkable Expe-"riment in *vacuo* in the following manner, "*viz.* He ſuſpended in the Receiver one "Thermometer, ſo as to be above the Froth "of the effervescent Mixture four or five "Lines of an Inch; the other Thermometer "he put into the Veſſel in which was a "Dram of *Sal Ammoniac*, ſuſpending over "it in a moveable Viol, three Drams of "Oil of Vitriol; then having drawn the "Air carefully out of the Receiver, he let "all ſtand thus for an Hour, that they might "have an equal Heat; after which on pour-

"ing

" ing the Oil of Vitriol on the *Sal Ammo-*
" *niac*, there was inſtantly a great Effer-
" veſcence, with ſuch Plenty of aſcending
" Fumes, as filled the Receiver, ſo that he
" could ſcarce diſtinguiſh the Degrees on
" the Thermometers, but in half a Minute
" the Receiver was more pellucid; the Li-
" quor in the Thermometer which was pla-
" ced in the effervefcent Mixture, had de-
" ſcended from ſixty ſeven to forty ſix De-
" grees in a Minute's time; after which it
" began to aſcend, and when it had come
" to fifty eight, then the Liquor in the o-
" ther Thermometer was at ſixty nine: and
" when the lower Thermometer was at ſixty,
" then the upper one was at ſixty nine one
" Quarter. But when after two Minutes the
" lower Thermometer was ſixty eight, then
" the upper was ſeventy; after another Mi-
" nute both Thermometers were at ſeventy:
" But after five Minutes the lower Thermo-
" meter was at ſeventy two, the upper one
" continuing at ſeventy. And the lower
" one aſcended to ſeventy four, even after a
" Quarter of an Hour; and when the Effer-
" veſcence ceaſed, yet the upper one con-
" tinued at ſeventy, the Effervefcence laſted

" at leaſt twenty Minutes. For greater Cer-
" tainty, he repeated this Experiment twice
" and found the Event the ſame. So the
" aſcending Vapours did in *vacuo* acquire
" three Degrees of Heat, the refrigerating
" Mixture twenty one Degrees of Cold; it
" at firſt increaſed in Degrees of Cold, but
" afterwards acquired Heat, as the Mixture
" abated of its Emotion; for while there
" was a great Effervеſcence ſo long the Cold
" continued. There was a remarkable Dif-
" ference between this Experiment in *vacuo*,
" and the other in the open Air, where the
" aſcending Vapour was remarkably warm,
" but in *vacuo* there was no Warmth given
" to the upper Thermometer till after the
" Effervеſcence was over, when the Fumes
" did not aſcend." Whence I ſuſpect that the Heat which the upper Thermometer then acquired, might be owing to the Heat communicated to it, from the Mixture below, which being at a Diſtance from it could not communicate its whole Heat, *viz.* ſeventy four Degrees: We may hence alſo conclude that in the open Air, the Action and Re-action of the Air, greatly increaſed the Effer-veſcence, and conſequently the Heat of the Fumes.

"78. The

"78. The ingenious Author obſerves hence
"the great Variety of very different Effects,
"which thoſe ſeveral effervefcent Mixtures
"produce, *viz.*

"79. As firſt, that the Effervefcences of
"the ſame Bodies produce the ſame Heat
"in the open Air and in *vacuo,* as appears
"by Number 48 and 49, when Crude An-
"timony and Spirit of Nitre were mixed
"together.

"80. Sometimes Effervefcences are greater
"and hotter in the Air than in *vacuo.*
"Thus Number 16. Biſmuth and Spirit of
"Salt made a greater Effervefcence in the
"open Air, and acquired a Heat from forty
"ſeven to a hundred and fifteen Degrees,
"whereas in *vacuo* the Heat was raiſed
"only from forty ſeven to ninety four De-
"grees. It was the ſame alſo in Number
"33 and 34, in 35 and 36, in 37 and 38, in
"forty five and forty ſix.

"81. On the contrary the Effervefcences
"are ſometimes hotter in *vacuo* than in the
"open Air, as is evident by comparing Num-
"ber 14 and 15, where Spirit of Sea Salt
"and Filings of Iron made a more vehement
"Effervefcence in *vacuo* than in the Air; for
"in

"in *vacuo* the Heat increaſed from forty
"ſeven to ſeventy, but in the Air, only from
"forty ſeven to fifty ſeven Degrees. It was
"the ſame in Number 24, in 27 and 28, in
"31 and 32, in 50 and 51, in 52 and 53, in
"63 and 64.

"82. With ſome Mixtures, though there
"be no viſible Efferveſcences in the open
"Air, there are great ones in *vacuo*, as in
"Number 1 and 2, in 50 and 51, in 52
"and 53.

"83. There are Efferveſcences which are
"hot in the Air, but acquire no Heat in
"*vacuo*, as was the Event in Number 43
"and 44.

"84. There are ſome Efferveſcences which
"produce a greater Degree of Cold in *va-*
"*cuo* than in the open Air, as in the Mix-
"ture of Spirit of Wine and Oil of Fennel
"Number 5.

"85. There are others which produce
"Heat in the open Air, but Cold in *vacuo*,
"as in the Mixture of Crabs-Eyes and Vi-
"negar Number 9, 10.

"86. There is ſometimes a great Heat ac-
"quired without a ſenſible inteſtine Motion,
"as on the Mixture of Oil of Vitriol and
"Water, Number 72, 73, 74.

"87.

" 87. There are Effervefcences which ac-
" quire neither Heat nor Cold, as Spirit of
" Sea Salt with Lead in *vacuo*.

" 88. There are alfo great Effervefcences
" which produce Cold, as Oil of Vitriol and
" *Sal Ammoniac*, as in Number 76, 77; it
" is the fame alfo with Oil of Vitriol and
" volatile Salt of Urine.

89. Whence our Author would infer, that thefe Degrees of Cold are acquired by the flying away of the Fire with the rifing Fumes; and that the Heat of thefe Effervefcences is not owing to the inteftine Motion of the Parts, but to a real inherent elemental Fire, on the flying away of which Cold is produced.

90. But when we confider, how vaftly great the attractive and repulfive Force of many of the Particles of Matter are, near the Point of Contact, it feems not improbable that the acquired Heat of effervefcent Mixtures may be owing to the inteftine Motion arifing from thofe contranitent and much agitating Powers; which as they are varied by infinitely different Combinations, fo their Effects muft needs be proportionably varied, fome Combinations tending to promote and greatly

greatly increaſe the efferveſcent Vibrations of the Particles, while others as much retard them: But as we cannot pry into the various Poſition of theſe Particles, in their ſeveral Combinations, on which their different Effects depend, ſo it will be difficult to account from any Principle, even though a true one, from the very different Effects of efferveſcent Mixtures.

"91. He further obſerves, that Bodies are "ſometimes diſſolved more by efferveſcent "Menſtruums in *vacuo* than in the Air: As "Lead is when thus mixed with Spirit of "Salt. And Filings of Iron mixed in the "ſame in Number 15. As alſo *Tutti* with "Spirit of Nitre Number 53.

"92. Yet Bodies are often more diſſolved "by a Menſtruum in the open Air, than in "*vacuo*, as Braſs in *Aqua fortis*.

93. He obſerves alſo, that there are frequently generated in Efferveſcences, either in the Air or in *vacuo* elaſtic Vapours, which are like Air; and are doubtleſs true permanent Air, for I have kept ſome of theſe thus generated Airs by me for ſix Years; after which, on compreſſing them in a condenſing Engine, in the ſame manner as is mentioned

oned in *Experiment* LXXVII. Vol. I. p. 191. I found that with equal Weights they were compreſſed exactly in the ſame Proportion with common Air, which was included with them in the Condenſer. I tried alſo Air which had been diſtilled from Rheniſh Tartar, the Day before, which I repeated again a Week after, in which time one fourth of the Air of Tartar had loſt its Elaſticity, as I found by the Degree of riſing of the Water up the inverted Tube, in which it was contained. That I might with the greater Certainty be aſſured of the Degrees of Compreſſibility of theſe different Airs, I divided the Capacities of two equal Tubes into quarter cubic Inches, by pouring ſeverally thoſe Quantities of Water into the Tubes, and then cutting Notches with a File, on the ſides of the Tubes at the ſeveral Surfaces of the Water; by which means I could ſee by the aſcent of the compreſſed Water in the Tubes, that both the factitious and common Air were exactly alike compreſſible in all Degrees of Compreſſure, from the beginning till they were loaded with a Weight equal to that of three Atmoſpheres, which was the fartheſt that I durſt venture, for fear of burſting the Glaſs Receiver.

94. And

94. And as to the noxious Quality of theſe factitious Airs, whether they are made by Fermentation, Effervescence, or Diſtillation; this cannot reaſonably be an Objection againſt their being true Air, becauſe it is well known that common Air is frequently alſo impregnated with moſt noxious and deadly Vapours. Thus the Fumes which aſcend up in the Air from fermenting Wines, are very pernicious, thoſe alſo from burning Brimſtone are moſt deadly; and ſuch Mr. *Hawksbee* found the common Air which paſſed thro' heated Iron and braſs Tubes, but the Air which paſſed through a heated Glaſs Tube was not noxious; an Argument that the Vapours which aroſe from the Iron and Braſs were noxious, and not the hot Air. There are many Inſtances in the abovementioned Experiments of Mr. *Muskhenbroek*, of Plenty of unelaſtic Vapours ariſing from the effervescent Mixtures in *vacuo*, moſt of which Vapours were doubtleſs very noxious, though they had no new generated elaſtic Matter among them, as was evident by the not ſubſiding of the Mercury in the Gage: Whence it ſeems probable, that the Noxiouſneſs of new generated Air, either by Fermentation,

Effervefcence, or Fire; as alfo of common Air impregnated with the like Fumes, is principally owing to thofe Fumes or Vapours, as mentioned in Experiment CXVI. Vol. I. and not to a rebating of the Degrees of its Elafticity, for that we fee is exactly the fame with that of common Air.

95. It is obfervable that though there was a very great Effervefcence made by many of the abovementioned Mixtures in *vacuo*, yet no elaftic Air was generated by feveral of them, and but little by many others, not near fo much as would probably have been made in the Bolt-head Fig. 34. p. 168. Vol. I. where I have given many Inftances of much greater Quantities of Air being generated by effervefcent Mixtures; the Reafon of which Difference feems to be this, *viz.* that the Action and Reaction of the common Air with the effervefcent Mixture in the Bolt-head, contributed to throw off more Air into an elaftic State, than was done in *vacuo* where there was none of that Air.

Experiment VI.

1. IN Experiment CXVI. Vol. I. p. 264. I have given an account of the reſult of ſeveral Experiments made by the Reſpiration of Air, included in Bladders. In order to obviate the Inconvenience that I apprehended might ariſe from the rancid Vapours of Bladders, I made uſe of the following Method, to try with greater Eaſe and Accuracy, how long I could breathe to and fro, a certain Quantity of Air, and to find how much of its Elaſticity was in that time deſtroyed, *viz.*

2. I cemented faſt at a Hole in the Top of an Air Pump-Receiver, a wooden Foſſet. I ſet this Receiver, whoſe Diameter was nine Inches, in a wide Veſſel which had two Inches depth of Water in it, leaving room at the Bottom for the Water to paſs freely to and fro. In this Poſition the Quantity of Air in the Receiver was equal to five hundred and twenty two cubic Inches. Then ſtopping my Noſtrils, I firſt breathed out what Air I could from my Lungs, and then applied my Mouth to the end of the Foſſet,

where I breathed to and fro, the five hundred and twenty two cubic Inches of Air, for two Minutes and half; when growing uneaſy, I breathed out all the Air I could from my Lungs, as at the firſt, at the ſame time giving a Signal to a Stander by, to mark the Height of the Water in the Receiver with a piece of Chalk; and upon meaſuring I found that eighteen cubic Inches, or $\frac{1}{29}$th Part of the whole Air, was in that time reduced from an elaſtic to a fixed State; but ſome further Allowance is to be made on account of the Expanſion of the Air, by reaſon of the Heat it had received in Reſpiration.

3. We ſee in this Experiment that near two Gallons of Air, which received no noxious Fumes from the Receiver, yet being breathed to and fro for two Minutes and half, became thereby unfit for Reſpiration: Whence no wonder that the Air ſhould be infected, and apt to breed Diſtempers in cloſe Priſons, where not only the Breath, but alſo the plentiful Perſpiration of many confined together ſtench the Air, and make it apt to breed what are called Goal Diſtempers; which Inconvenience might in a good Mea-

ſure be prevented, if Goals were ſo contrived as to have a free Paſſage for the Wind to blow through them, and thereby communicate freſh Air, for want of which many of thoſe unhappy Perſons are not only deprived of Liberty in Goals, but too often even of Life alſo.

4. I have been informed by a Perſon long experienced in maritim Affairs, that when the Air between Decks has grown offenſive by the Vapours ariſing from the Bodies of many Perſons, that it has been found of ſome Benefit towards purifying that Air, to waſh the Beams of the Decks with Vinegar, and to ſprinkle it about; and accordingly in *Experiment* CXVI. Vol. I. p. *266.* it was found that an Air which paſſed through Cloths dipped in Vinegar could be breathed to and fro as long again as the like Quantity of Air which was not thus purified; ſo that the ſprinkling of Vinegar between Decks might a little refreſh the Air; yet where the Stench is great it can be of but little Benefit, and that only for a ſhort time; nothing but a thorough ventilating Air can then be an effectual Cure. Vinegar has been long looked on as anti-peſtilential; whence it is probable,

that there may be a Ferment between this Acid, and the then too alkaline Air, which may thereby be reduced in ſome Degree from its alkaline, to a neutral, more wholſome State, for many acid and alkaline Mixtures produce neutral Liquors.

5. Now I found in the following manner, the Quantity of Moiſture with which theſe five hundred and twenty two cubic Inches of Air were impregnated, by being breathed to and fro, *viz.* I broke off the Neck of a *Florence* Flask, ſo low, that the Diameter of its Orifice was three Quarters of an Inch, and then filled it within an Inch of the Top, with Wood Aſhes well burnt: Then I run a Glaſs Tube down to the Bottom of the Aſhes, and covered the Mouth of the Flask with a fine Rag, to prevent the Aſhes being blown out, when I breathed through them. Then pinching my Noſtrils cloſe, I breathed through the Glaſs Tube, which conveyed my Breath to the Bottom of the Aſhes; they being very dry and ſcarce cold, imbibed by their lixivial Salt, the Moiſture of my Breath as it paſſed through them; and having exactly poiſed the Flask and Aſhes in a Scale, before I began to breathe through them, I

 found

found that after having breathed fifty times through them, the Aſhes had increaſed in Weight ſeventeen Grains. The Air which I there inſpired was very dry, for there were conſtant Fires in the room; ſo that the increaſe of Weight which the Aſhes acquired, muſt come from the Moiſture carried off from my Lungs. And this may be taken to be nearly the Quantity of Moiſture with which the abovementioned five hundred and twenty two cubic Inches of Air were impregnated, when it became unfit for Reſpiration; for we breathe fifty times in two and a half Minutes. Now a cubic Inch of Water weighing two hundred and fifty four Grains, the five hundred and twenty two cubic Inches will weigh 132588 Grains, whence the like bulk of Air, which is eight hundred times lighter, will weigh 165.7 Grains, of which the abovementioned ſeventeen Grains of Moiſture is but $\frac{1}{98}$th Part. Now this Proportion of additional Moiſture in the Air, does not ſeem alone ſufficient to diſqualify it for Reſpiration: For the common Air has frequently a greater Proportion of Moiſture in it, *viz.* a Quantity equal to the one third, and ſometimes to one half of

the Weight of the Air, which paſſed thro' burnt Aſhes into an exhauſted Receiver, tho' in a dry Summer Air there was no Moiſture found by this Means, as Mr. *Peter Van Muskhenbroek*, who made the Experiment, relates in his *Oratio de Methodo inſtituendi Experimenta Phyſica*, p. xxviii. *Vide Tentamina Experimentorum naturalium captorum in Academia del Cimento.* We may therefore hence reaſonably conclude that theſe five hundred and twenty two cubic Inches of Air were diſqualified for Reſpiration, not barely by ſuch an additional Moiſture, but alſo from ſome ill Qualities in that Moiſture; I ſuſpect among others, the Groſſneſs of theſe Exhalations from the Lungs, which may hinder their free entrance into the minute Veſicles; for it is with very great Difficulty, that the almoſt ſuffocated Lungs can, in this Experiment, be but a little dilated.

6. We may alſo from this Experiment on Wood-Aſhes, eſtimate the Quantity of Moiſture that is carried off by Reſpiration; for ſince a Quantity equal to ſeventeen Grains was breathed off in fifty Expirations, there will proportionably be four hundred and eight Grains evaporated in the twelve hundred Expirations

pirations of an Hour; and in twenty four Hours 9792 Grains, or 1. 39 Pound; which ſuppoſing the Surface of the Lungs to be as found in Vol. I. p. 242. 41635 ſquare Inches, then the Quantity evaporated from that inward Surface will be $\frac{1}{1074}$th part of an Inch depth.

EXPERIMENT VII.

1. THE Conſideration of the continued Succeſſion of freſh Air, which is found neceſſary to keep a Fire burning; alſo of the intenſe Degree of Heat which a Fire ſoon acquires by being vigorouſly blown on by Bellows; theſe Conſiderations put me upon trying by Experiment what the Velocity and Force of that Air might be; which I found by applying a proper mercurial Gage to the Noſe of a Pair of Smith's Bellows; whereby I found the Force of the compreſſed Wind in the Bellows, was ſufficient to raiſe the Mercury in the Gage about an Inch high, ſometimes a little more and ſometimes a little leſs. Whence the Force with which the Bellows impel Air into the Fire, is nearly equal to $\frac{1}{30}$th Part of the Weight of the Atmoſphere,

ſphere, which Force does, as we ſee, make the compreſſed Air ruſh out of the Bellows with great Velocity.

2. Which Velocity I eſtimated in the following manner, *viz.* by meaſuring the Surface of the upper Board of the Bellows, and alſo the Space they deſcended in a Second of time; whence having got the Quantity of Air expelled in that time, *viz.* four hundred and ninety five cubic Inches, this divided by the Area of the Orifice of the Noſe of the Bellows gave eight hundred and twenty five Inches, or 68. 73 Feet, the Length of the Cylinder of Air, which ruſhed out of the Bellows into the Fire in that time. Which Air being compreſſed with a Weight $\frac{1}{30}$th Part greater than the Weight of the Atmoſphere, its Quantity was $\frac{1}{30}$th Part of 495 cubic Inches greater, *viz.* 16. 5 Inches, in all 511. 5 cubic Inches of Air; which being thus forcibly impelled into the Fire with a Velocity of 68. 73 Feet in a Second of time, the Action and Reaction between the elaſtic vibrating Matter of the Fewel, and that of the Air, being thereby ſo greatly increaſed, as to cauſe a moſt intenſe Heat, ſufficient to melt Metals.

The

The Force with which the Bellows of an Organ impel Air into its Pipes, being taken in this manner, might not the Velocities of the Undulations of Air, which are requiſite to form thoſe Sounds, be thence eſtimated? The Velocity of undulating Air to that of Water is as eight hundred ſixty five to one, *viz.* nearly as their ſpecific Gravities.

A DE-

A DESCRIPTION OF A SEA-GAGE,

Wherewith to Meaſure unfathomable Depths of the Sea.

1. IN my firſt Volume under *Experiment* LXXXIX. p. 209, I propoſed a Method for finding out the Depth of the Sea at unfathomable Depths; which Method the ingenious Dr. *Deſaguliers* put in practice before the Royal Society, with a Machine which he contrived; the Deſcription of which he has given in the *Philoſophical Tranſactions*, Number 405. I ſhall here more particularly deſcribe how to prepare and graduate this Sea-Gage.

2. Suppoſe

2. Suppoſe there be an Iron or Copper Tube, or a Musket Barrel of any Length, as fifty Inches, having its upper end well cloſed up: If this Tube be let down, in this Poſition, about thirty three Feet in the Sea, for a Column of Sea Water of that Height is nearly equal to the middle Weight of our Atmoſphere; then conſequently from a known Property of the Air's Elaſticity, it will be compreſſed into half the Space it took up before, ſo that the Water will aſcend half way up the Tube, and if the Tube be let down thirty three Feet deeper, the Air will be compreſſed into one Third of its firſt Dimenſion, and ſo on $\frac{1}{4}$th, $\frac{1}{5}$th, $\frac{1}{6}$th, *&c.* The Air being conſtantly compreſſible in Proportion to the incumbent Weight; whence by knowing to what Height the Water has aſcended in the Tube, we may readily know to what Depth the Tube has deſcended in the Sea.

3. Now to meaſure the Depth of one of theſe Columns of Sea Water, whoſe ſpecific Gravity to pump Water is found to be as forty one to forty. Firſt by a Line let the Iron Tube, with a Weight at its Bottom, ſink about thirty three Feet, which Depth in Salt

Salt Water will nearly anſwer to the Weight of the Air, at a mean Height of the Barometer, then draw up the Tube and obſerve how far the Water roſe ; if thirty three Feet of Water is equal to one Atmoſphere, then will the Water riſe ſo high as to fill exactly one half of the Tube : But if the Water riſes higher or lower than half way, then by the Rule of Three ſay, as the Number to which the Water riſes is to one, ſo is thirty three to the Number of Feet, meaſuring the Depth of the Column required. For Example, ſuppoſe the Water riſes (when the Tube is let down thirty three Feet) only nine Tenths of half way, then ſay as $9 : 10 :: 33 : 36\frac{1}{2}$ Feet the Depth of each Column, which being once known, the Number of Columns of Water is to be multiplied by this Number of Feet, whereby the Depth of the Sea in Feet will be known.

4. But ſince when the Inſtrument has deſcended to the Depth of ninety nine Columns, or ninety nine times thirty three Feet, the Air will be compreſſed into the $\frac{1}{100}$th Part of fifty Inches, that is, half an Inch, the Diviſions both for ſome Space below,

low, and alſo above that, will be ſo very ſmall, that the Difference of ſeveral Columns Depth of Water will not be ſenſible: So that an Inſtrument of no greater Length than this, would ſcarcely give an accurate Eſtimate of half a Mile's Depth, that is, 2640 Feet, or eighty Columns depth of Water. The lengthening therefore of this Inſtrument to four, five, or ten times its length, would obviate this Defect, and make the Difference of the Degrees of Deſcent more ſenſible on the Inſtrument. But ſince it is impracticable to make a metaline Tube of ſo great a Length, and if it were made, it would be ſo unweildy as to be eaſily broken; it is propoſed therefore to obviate theſe Difficulties by the following Method, *viz.*

5. Let there be a globoſe metaline Body be made of Copper, whoſe Capacity within ſide may be equal to nine times the Capacity of the metaline Tube: Let this globoſe Body be firmly ſcrewed to the metaline Tube with a leathern Collar, well ſoaked in ſome unctuous Matter, thereby to ſecure that Joint in the moſt effectual manner.

6. There muſt be a Hole at the Bottom of the Copper glob Opening into a metaline Tube ſoddered to that Hole, which Tube muſt be three or four Inches length, that it may be immerſed in a proper metaline Ciſtern, full of ſome tinged unctuous Matter, as Oil, or the like, which being ſpecifically lighter than Sea Water, will ſwim upon it, and conſequently will daub the inſide of the Iron Tube, as far as the Water was impelled up it. And in order to know that Height, there muſt be fixed a ſlender Rod of Braſs, Iron or Wood, in the middle of the Iron Tube from End to End, with a Knob at its upper End, both to keep it at that End in the middle of the Tube, and alſo to prevent its being beſmeared by the ſides of the Tube, when it is drawn out to ſee by the Mark of the unctuous Matter on the Rod to what Height the Water had been preſſed up the Tube.

7. The Capacity of the Tube muſt be eſtimated by pouring Water in, when the Rod and metaline Diameter are fixed in their Places.

8. Now ſince the Copper Globe is ſuppoſed to contain nine times as much Air as the

the Tube, which is the ſame thing as if the Tube was nine times as long, therefore the Air in the globoſe Veſſel, will not be forced within the Capacity of the Tube, till the Veſſel has deſcended to the Depth of nine Columns, or nine times thirty three Feet; for then the Air will be compreſſed within one tenth of the Space it at firſt took up.

9. Suppoſing therefore the Inſtrument to have deſcended to the Depth of ninety nine Columns of Water, or ninety nine times thirty three Feet, *viz.* 3267 Feet, then the Air will be compreſſed within $\frac{1}{100}$th part of five hundred Inches (for the Capacity of the whole Veſſel was ſuppoſed equal to a Tube of that length) that is, within five Inches of the Top of the Tube; and conſequently the Rod will be found tinged with the coloured Oil within five Inches of its Top.

10. Suppoſe again the Inſtrument to have deſcended to the Depth of a hundred and ninety nine Columns of thirty three Feet each, then the Air will be compreſſed within the $\frac{1}{200}$th part of the whole, that is, nearly within two and half Inches of the Top of the Tube. In this Caſe the Inſtrument will

have deſcended 6567 Feet, that is, a Mile and a Quarter, and a hundred and thirty two Feet.

11. Suppoſe again the Machine to have deſcended to the Depth of three hundred and ninety nine Columns, then the Air will be compreſſed within $\frac{1}{400}$th part of the whole, that is, nearly within one Inch and Quarter of the Top of the Tube; in this Caſe the Machine will have deſcended two Miles and half, wanting fifty three Feet; which may probably be the greateſt Depth of the Sea.

12. But if there were occaſion to explore greater Depths than this, it might we ſee be done with tolerable Accuracy, by enlarging the Capacity of the globoſe Veſſel, which might be done without making it very cumbrous. For ſuppoſe the Diameter of the Tube were about $\frac{3}{4}$ths of an Inch, *viz.* common Musket Barrel bore, and that it were fifty Inches long, if the globoſe Veſſel were nineteen times as big, it would not in that caſe exceed the Bulk of three Gallons. But the bigger the globoſe Veſſel, the greater care muſt be taken to ſecure well the Screw where

it

it is joined to the Tube, that no Air paſs that way.

13. The bigger the globoſe Body is, the more weighty it ought to be, thereby the more effectually to keep it in a low depending Poſture; elſe the Buoyancy of its contained Air might raiſe it as high or higher than the upper part of the Machine, whereby Water ruſhing in to the Top of the Tube no Obſervation could be made, the Rod being thereby moiſtened from End to End. When one Experiment has been made, the Rod and Tube muſt be wiped very clean, before another be repeated.

14. This Sea-Gage being thus prepared, a large Buoy muſt be fixed to it, which ought to be a large piece of ſolid Fir, or any other light ſolid Wood, well covered with Tar, to prevent any Waters being preſſed into the Sap-Veſſels; for I have found by Experience that Wood which was conſiderably lighter than Water, has by being greatly compreſſed in Water, become immediately ſpecifically heavier than Water, for the conſtituent Parts of all Vegetables are ſpecifically heavier than Water. If the Buoy be made of a Bladder or hollow Globe, with its Ori-

fice inverted downwards, the Air in them will be compreſſed to ſuch a Degree, at great Depths, as thereby to make the buoyant Body become ſpecifically heavier than the Sea-Water, which would prevent its reaſcending to the Surface of the Sea; for which reaſon alſo the Buoy ought to be able to buoy up the Inſtrument when full of Water. Beſides, if the Buoy when it riſes again do not appear ſome conſiderable Height above the Water, it will not eaſily be diſcovered: For it is probable, that from great Depths it may riſe at a conſiderable diſtance from the Ship though in a Calm. To make the Buoy therefore the more viſible at a Diſtance, it will be proper to fix acroſs each other, at the Top of the Buoy, broad Fans of Tin, painted either black or white, as ſhall be found moſt convenient.

15. For greater Accuracy, it will be needful firſt to try this Sea-Gage, at ſeveral different Depths, down to the greateſt Depth that a Line can reach, thereby to diſcover, whether or how much the Spring of the Air is diſturbed or condenſed, not only by the great Preſſure of the incumbent Water, but alſo by its Degrees of Warmth or Coldneſs

neſs at great Depths, and in what Proportion at different known Depths, and in different lengths of time, that an Allowance may accordingly be made for it at unfathomable Depths.

16. And becauſe it is probable that the Temper of the Air, when the Experiment is made, will be either warmer or colder, than that of the Sea at a conſiderable Depth, it will therefore be adviſeable to let down the Inſtrument with a Line to a good Depth, there to continue for ſome time, till the Air in it may be ſuppoſed to come to the ſame Temper with the Sea Water: Then the Machine is to be pulled up ſo far above Water, as to let the Air freely paſs either in or out of the Globe and Tube, according as the included Air ſhall either have dilated or contracted. Then inſtantly let the Machine looſe to drop down to the Bottom of the Sea: Which it will do by means of a ſinking Weight of Ballaſt, which muſt be fixed in the following manner, *viz.*

17. The ſinking Weight of Ballaſt muſt be ſo fixed to the Machine, by means of a Catch-Hook, that as ſoon as the Weight touches the Ground at the Bottom of the

Sea, the Catch may then by means of a Spring, let go its hold; whereby the Buoy will be at Liberty to carry the Machine up to the Surface of the Water.

18. The Weight of the ſinking Ballaſt ought to be ſo proportioned, as to be juſt ſufficient to ſink the Machine at firſt, for as the Machine deſcends it grows continually ſpecifically heavier, by reaſon of the Condenſation of the Air in its Cavity, on which account its Motion will be accelerated, as well as on account of the inceſſant Action of the Power of Gravity upon it, ſo that if this gravitating Power far exceeded the contrary Renitency of the Buoy, it would ſtrike the Bottom of the Sea with ſo great a Force, as might endanger the breaking of the Machine.

19. It would therefore be adviſeable firſt to let down the Buoy with ſomething of equal Weight with the Machine, and an Iron Rod intervening between the Machine and the Buoy, thereby to gueſs by the bending or not bending of the Rod, with what Degree of Force it might ſtrike the Bottom of the Sea. And if the Force ſhould be found to be great, I believe it might be adviſeable to

fix

fix a Pole between the Machine and the Ballaſt, of ſuch a Degree of Strength, that it would break before it could give Reſiſtance enough to hurt the Machine; this would greatly break its Force againſt the Ground. Some of the Sand or Earth at the Bottom of the Sea ſhould be brought up as in common Soundings, with Tallow at the Bottom of the Plummet.

20. It would be adviſeable alſo to keep an exact Account of the time of the Machine's ſtay under Water, which may be done by a Watch that beats Seconds, or by a Pendulum vibrating Seconds, which muſt be three Feet three Inches and one fifth of an Inch long, between the middle of the Bob and the upper end of the Line.

21. Dr. *Hook* in the *Philoſoph. Tranſact. Lowthorp's Abridgment*, Vol. 2. p. 258. found upon trial that a leaden Ball which weighed two Pounds, being fixed to a wooden Ball of the ſame Weight, and both let down in fourteen fathom Water, they reached the Bottom in ſeventeen Seconds, and the detached wooden Ball aſcended to the Surface of the Water in ſeventeen Seconds more; ſo that if the Machine above deſcribed deſcend-

ed and aſcended to greater Depths with the ſame Velocity, it would reach to the Depth of a Mile in ſeventeen Minutes, and reaſcend in the like time. But ſince the buoyant Body may return faſter to the Surface of the Water than it deſcended, therefore Eſtimates from the time of the Bodies keeping under Water will be very uncertain: Yet when frequently compared with the Eſtimate which is made, from the height of the Water in the Gage-Tube; a Rule may perhaps be formed from thence, eſpecially if the whole Machine be always the ſame, and the ſinking Ballaſt be always of the ſame Weight and Size: As ſuppoſe the Ballaſt were put into globular earthen Veſſels, made all of the ſame Diameter.

22. That the Sea is not many Miles deep is probable from hence, that all the great Oceans are here and there interſperſed with Iſlands; an Argument that though as far as the ſounding Line has reached, the Sea is found to be deeper and deeper, the farther from the Shore (though with ſome Unevenneſs) which would come to a great Depth indeed if it continued on ſo, from one boundary of the vaſt Oceans to the other; but the

the interſperſed Iſlands prove that it is not ſo.

23. If we ſuppoſe the Sea to deepen from the Shores in nearly the ſame Proportion that Land riſes from the Shores, then from the following Eſtimate the greateſt Depth of the Sea will not exceed five or ſix Miles. For ſince ſlow Rivers are found to have a Fall of about a Foot in a Mile, if we ſuppoſe the River *Niger* in *Africa* (which is one of the longeſt Rivers in the World, and runs about 2400 Miles in length) to fall at the rate of four Feet each Mile, then its whole Fall from its Riſe, to its diſcharging itſelf into the Sea will be 1. 81 Miles; if it falls at the rate of ſix Feet each Mile, then its whole Fall will be 2. 72 Miles. If eight Feet each Mile, its Fall will be 3. 72 Miles. But if the Fall be ſet at ten Feet each Mile, then the Fall of the River will be 4. 54 Miles, which is a large Allowance, and may therefore well include the height of the Tops of the Mountains, from whoſe ſides thoſe Springs break forth: The higheſt Mountains being eſtimated to be ſcarce $\frac{1}{859}$th of the Earth's ſemidiameter.

If

If we ſuppoſe that the whole Quantity of Earth, which is above the level of the Surface of the Seas, were equal to the whole Bulk of the Waters in the Baſin of the Seas; then ſince the Sum of the Expanſe of all the Seas is conſiderably more than the Sum of the Surface of all the Earth on this Globe, the general Depth of the Sea muſt therefore be conſiderably leſs, than the general Height of the Earth above the Surface of the Sea.

24. To this Method of taking the Depth of the Sea it may reaſonably be objected, that though it has been found by Experience that in leſſer Degrees the Air is compreſſible in Proportion to the incumbent Weights; yet it is to be feared that in greater Degrees of Compreſſure, this proportional Compreſſibility would not hold true, by reaſon of the many watry and other heterogeneous Mixtures in the Air, which would hinder its equable Compreſſibility. Yet ſince the irregular Degrees of its Compreſſibility are not yet certainly known, it might be well to try the Inſtrument firſt to the greateſt Depth a Line can be let down, which I find has been to four hundred Fathom, in which caſe the Air

Air would be compreſſed with more than ſeventy two Columns of Water, each equal to the Weight of the Atmoſphere, ſo that the Air would be compreſſed into $\frac{1}{73}$d part of its natural Bulk, in which caſe the Denſity of that Air would be to Water as 1 to 11.64. When the Air is compreſſed with the Weight of ninety nine Columns of Water at the Depth of 3267 Feet, or half a Mile and 627 Feet, then its Denſity will be $\frac{1}{8}$th of Water; at a hundred and ninety nine Columns Depth, or a Mile and Quarter, and a hundred and thirty two Feet, its Denſity will be $\frac{1}{4}$th of Water; and at three hundred and ninety nine Columns depth, or two Miles and half wanting fifty three Feet, its Denſity will be $\frac{1}{2}$ of Water.

25. I have compreſſed Air with a Weight equal to 37. 44 Atmoſpheres in the following manner, *viz.* I took a Glaſs Tube which was cloſed at one End, the Length of its Cavity 4. 06 Inches, its Diameter 0. 16. being firſt counterpois'd in a Scale, and then filled with Water, the Weight of the Water it contained was one Dram and ſix Grains. The open End of this Tube I immerſed in a ſmall Viol, at the Bottom of which was ſome

Mercury with a little Spirit of Turpentine, which was tinged with Indigo: The Viol and Tube were let down into a large Bomb full of Water, which was placed under a Cyder Preſs; then there was put into the Orifice of the Bomb a well turned Plug of Holly Wood, which was impelled ſo forcibly into the Bomb by the Screw of the Preſs, that Water ouzed through the Pores of the Plug, notwithſtanding it had been dipped in melted Cement made of Bees Wax and Turpentine. When I took the mercurial Gage out of the Bomb, I found the tinged Turpentine had riſen ſo high as to have compreſſed the included Air within the Space of 0. 12 Inch of the Top of the Tube; that is, the 38. 44th part of its Capacity; ſo that the included Air was compreſſed with 37. 44 Atmoſpheres, equal to the Preſſure at 1235. 5 Feet or 205. 9 Fathom depth in the Sea: So that the Denſity of this Air was to that of Water as 1 to 22. 7.

26. Air when thus greatly compreſſed, has not been obſerved either to enter the Mercury in the Gage, nor to paſs through the Pores of the Glaſs; nor has it been found to be fixed by any known Degrees of Com-

preſſure

preſſure or Cold. What Effect ſuch an extream Preſſure as two or three Miles depth of Sea Water would have on it, we can know only by Experience; which might probably be tried in the manner here propoſed without much Difficulty.

27. The greateſt Degree of Compreſſure that I have been able to give Air was in the following manner, *viz.* Having in froſty Weather placed the abovementioned Bomb with the mercurial Gage in it under the Cyder Preſs in the ſame manner as before deſcribed, I then covered the Bomb with a large Quantity of pounded Ice, which had half its Quantity of common Salt mixed with it: In a little time this great Degree of Cold burſt the Bomb into three Pieces, from Top to Bottom; theſe Pieces opened wide, by falling aſunder, but their lower Parts touched each other; an evident Proof that the Water, though compreſſed to ſo great a Degree, as to burſt the Bomb, yet had very little Elaſticity in it.

28. The Bomb was lined all over within ſide with a Shell of Ice which was about $\frac{1}{4}$ths Inch thick, it was full of Air Bubbles.

29. As

29. As to the Viol and mercurial Gage they were broken into many Pieces, and all the Pieces of the Tube or Gage were dawbed within ſide with Turpentine and Particles of Mercury, to the very Top of the Tube, which was frozen at each End into the Shell of Ice. As the Water in the middle of the Bomb was not frozen, this Experiment might eaſily be repeated without Danger of breaking the Gage, or Viol, *viz.* by fixing them to a ſmall Stick, of equal Length with the Diameter of the Bomb, by which means they would be ſuſtained within the Limits of the unfrozen Water.

30. We may from this Experiment make ſome Eſtimate of the Force which was requiſite to burſt the Bomb, and conſequently of the Degree of Compreſſure which the Air in the Gage ſuſtained, *viz.* The Diameter of the Bomb within ſide was $6+\frac{1}{2}$ Inches, the Thickneſs of its Subſtance at its Orifice 1. 2 Inch, its Thickneſs at the Bottom was 1. 9 Inch. Now ſuppoſing the Subſtance of the Bomb to be all over of the ſame Thickneſs with its thinneſt part, *viz.* 1. 2 Inch; then the Area of the Ringlet which cuts that Subſtance tranſverſely in its biggeſt Circle, will

will be equal to 29. 72 ſquare Inches: That I may therefore make ſome Eſtimate of the Degree of Coherence, of the Subſtance of the Bomb in this Ringlet, I ſhall found my Calculation on Mr. *Muskhenbroek's* LXXVIIth Experiment, in his *Introductio ad Cohærentiam Corporum*, p. 505. where he found that Iron Wire, whoſe Diameter was $\frac{1}{10}$th of a *Rynland* Inch, was pulled aſunder with four hundred and fifty Pounds weight. The Wire being made of hammered Iron, was probably tougher than the Bomb which is made of caſt Iron; I have therefore made a large Allowance for this, in leaving a great part of the Thickneſs of the Bomb out of this Eſtimate. A *Rynland* Inch being to an *Engliſh* Inch as 0. 752 to 1, then $\frac{1}{10}$th of the *Rynland* Inch is 0. 0752, to which 0. 133 of an *Engliſh* Inch being equal, it expreſſes the ſame Diameter of the Wire, the Area of whoſe tranſverſe Section is therefore 0. 013, by which Number, if the abovementioned Area of the Ringlet, *viz.* 29. 72 be divided, the Product is 2286, which multiplied by 450, the Weight which was requiſite to pull the Wire aſunder, the Product is 1028700 *Amſterdam* Pounds, which being to common-

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mon Avoirdupoiſe Weight as 93 to 100, will amount to 956690 Pounds Avoirdupoiſe, equal to the Force requiſite to burſt the Bomb in halves. And the Area of the greateſt Circle within the Bomb being equal to 33. 16 ſquare Inches, and fifteen Pounds five Ounces being nearly equal to the Weight of the Atmoſphere on a ſquare Inch, theſe multiplied into 33. 16 ſquare Inches give 504. 3 Pounds, the Weight of the Atmoſphere on a Circle of that Area; by which dividing the above Number 956690 the Product 1837, gives the Number of Atmoſpheres, which preſſed on the Air in the Gage; which Air muſt therefore be compreſſed into $\frac{1}{1838}$th part of its firſt Space: Which is equal to the Weight of a Column of Sea Water ſo many times thirty three Feet high, or 60654 Feet or eleven Miles. And the length of the Cavity of the Gage being 4. 06 Inches, if the Air in it was condenſed, in Proportion to the incumbent Weight, it was then compreſſed into 0. 0022 part of the Length of the Tube, or nearly $\frac{1}{500}$th part of an Inch; too ſmall a Space to be ſeen.

31. And

31. And ſince as was above obſerved in Experiment III. Number 13. the Air with the Preſſure of one Atmoſphere, ruſhes into a *Vacuum*, at the Rate of 1305 Feet in a Second of Time, with the Impulſe of theſe 1837 Atmoſpheres, it would in *vacuo* have a Velocity of four hundred and fifty four Miles in a Second. And how much greater muſt the Velocity of expanding Gun Powder be, which moves with ſo irreſiſtible an Impetus.

32. In order to eſtimate how much the Shell of the Bomb might contract by the Coldneſs of this freezing Mixture, I layed an Iron Back of a Fire Stove, which was of caſt Iron, on a Mixture of pounded Ice and Salt, and covered it with the ſame, the Cold made it contract $\frac{1}{8}$th of an Inch, i. e. $\frac{1}{120}$th part of its Length, which was twenty five Inches. Whence by Calculation I found, the Capacity of the contracted Shell was 0.263 cubic Inch leſs, *viz.* $\frac{1}{547}$th part leſs, than that of the ſame uncontracted Shell. And the Shell of Ice within ſide, at the ſame time dilating about $\frac{1}{10}$th of its Bulk, the Bomb muſt neceſſarily burſt.

 33. In

In order to try the Incompreſſibleneſs of Water, I filled the abovementioned Bomb, before it was burſt, with Pump Water, well purged of Air, which had ſix + ½ Degrees of Coldneſs above the freezing Point: Then placing it under the Cyder Preſs, I preſſed faſt into its Orifice a Plug, which had a Hole through it from Top to Bottom of about half an Inch Diameter; into which Hole I drove with a Hammer a ſtrong tough aſhen Pin, which was covered with Cement: When the Pin which was half an Inch Diameter was ſo faſt drove in that no Water could paſs by it, it then met with ſo abſolute a Reſiſtance from the Water, that it ſeemed as if the lower end of the Pin had been drove againſt a Stone or ſolid Iron; ſo that on ſtriking very hard Blows with the Hammer, the Pin was bruiſed to pieces between the Hammer and the reſiſting Water. And the Diameter of the Bomb within ſide, being ſix + ½ Inches, the Area of its greateſt Circle is 33. 06 ſquare Inches, then the inward Surface of the Bomb will be four times that Area, *viz.* 132. 24 ſquare Inches: This divided by 0. 196 ſquare Inch, the Area of the Bottom of the aſhen Pin, the Product

674. 7

674. 7 expreſſes the Number of Stroaks with which all ſides of the Bomb are ſtruck, at the ſame Inſtant and with the ſame Force that the Hammer ſtrikes the aſhen Pin, for compreſſed Fluids preſs equally on all ſides: And as theſe Stroaks of the Hammer would ſoon have diſtended and burſt a Sphere of ductile Silver, ſo the Water was by this means much more forcibly compreſſed in this Bomb than in the Silver Sphere of the *Academy* of *Cimento.*

OBSERVATION XII.

1. DR. *Plot* in his Hiſtory of *Oxfordſhire* obſerves, that Rivers are found conſtantly to freeze firſt at their Bottoms: And the ſame is alſo obſerved by Watermen and Fiſhermen, in the *Thames* both within the reach of the Tide and alſo above the Tide; who not only feel it at the Bottom with their Poles, a conſiderable time, *viz.* ſome Days before the Surface of the *Thames* is frozen over, but ſee it alſo riſe up from the Bottom with ſuch Force, as to dart up in pieces edgeways half a Foot, and ſometimes a Foot above the Surface of the Water, in which Poſture it continues a little time, and then turning flat on the Surface of the Water, it is carried in

great Quantities along the Stream, and is called *Ice-Meers*, which if the Froſt continues, harden into one ſolid Surface when the River is frozen over.

2. *Jan.* 30, 1730. The Spirit in the Thermometer which hung in the open Air, being at ſeven a Clock in the Morning twelve Degrees below the freezing Point, and there having fallen in the Night near an Inch depth of Snow, I went to the *Thames*, and found, in a Bay at the watering Place at *Teddington*, where there was very little Stream, the Surface of the Water frozen about $\frac{1}{3}$th of an Inch thick, under which Ice, I ſaw at the Bottom another Bed of Ice; then breaking away ſome of the upper Ice with a Paddle, I took up ſome of the lower Ice, which was about half Inch thick, but was not ſo ſolid as the Surface Ice, but more ſpongy and cavernous. This lower Ice joined to the upper Ice at the brink of the Water, and was gradually more and more diſtant from it, as the Bottom deepened; for it adhered cloſe to the Bottom, where the Stones and Sand were incorporated into it, and which it brings up to the Surface along with it, when it is frozen to ſuch a Degree,

as

as to be ſo much more ſpecifically lighter than Water, as to be able to buoy them up with it. And when it freezes to a conſiderable Thickneſs, it will raiſe up with it from the Bottom, the Fiſhermens Oſier Wheels which are ſunk down with Stones or Bricks tied to them to catch Fiſh.

3. *Dec.* 28, 1731. At eight a Clock in the Morning, the Spirit in the Thermometer being then $12+\frac{1}{2}$ Degrees below the freezing Point, I found the ſame Bay frozen over in like manner, both on the Surface and at the Bottom, as far as to the Edge of the quick Stream, where it was not frozen either at the Bottom or on the Top, its Motion being too brisk: And accordingly the Watermen and Fiſhermen obſerve, that it freezeth ſooneſt at the Bottom where the Stream is moſt gentle; and the ſame may be obſerved as to the Surface of a Pond, which will be frozen over ſooner, in that part on which a gentle North-Eaſt Wind blows, than where it is more expoſed to a ſtronger Wind.

4. Though Snow in froſty Weather haſtens the freezing of Water, yet it is obſerved that the *Thames* freezes firſt at the Bottom alſo when no Snow has fallen long before.

So that this cannot be owing to the ſinking of the Snow to the Bottom of the River.

5. Now ſince this freezing at the Bottom is never obſerved, in Ponds and perfectly ſtagnating Waters, it muſt therefore be owing to the Motion of the Stream; for in ſtagnant Waters, as well as on the Earth, it is certainly colder in froſty Weather at their Surfaces than at ſome Depth; whereas in a flowing Stream, the upper and lower Waters being continually blended together, they are thereby brought nearly to an equal Degree of Coldneſs, and the upper Water being at the ſame time in a greater Degree of Motion than the lower, it cannot ſo ſoon freeze; for in this Bay, where the Motion of the Water was ſmall, its Surface was frozen over, as well as the Bottom, though not with ſo thick an Ice: Whereas in the main River where its Motion was greater, its Surface was not frozen over, notwithſtanding it was much overſpread with Ice-Meers, which were continually riſing from the Bottom.

6. Another Cauſe which may contribute to a River's freezing firſt at the Bottom, may be this, *viz.* That as by Experience it is found much colder to ſtand expoſed to a bleak

bleak Stream of ſharp Air, than to ſtand in the ſame open Air under the Shelter of a Hedge or Wall; and as the Hand put into cold Water, will be ſenſible of a greater Degree of Cold, if the Hand be moved to and fro in the Water, than if it be held ſtill; becauſe in both theſe Caſes, the ſtagnant Fluids which immediately touch the Body, acquire by a conſtant Touch a Degree of Warmth, more nearly approaching to that of the Body; whereas when there is a continued Succeſſion of cold Fluid ſtriking on the Body, it muſt needs then be affected with a greater Degree of Cold. And the Difference is the ſame between a River and a Pond, the Ground at the Bottom of the River being reduced to a much colder State, by the continual flow of Water over it, than the Bottom of a Pond whoſe Water is ſtagnant. And in Confirmation of this, it is obſerved, that it freezes firſt at the Bottoms, where the Stream is gentle, but not at all at the Bottom of deep Holes, whoſe Depth ſhelters them from the continued Courſe of the Stream: For which Reaſon the Fiſhes are obſerved to retreat into the deeper Places in very cold Weather.

7. And for a further Confirmation of this, there was juſt by the Bay where I made the Obſervations, a narrow Creek or Dock, as broad as two ſmall Boats, and as long as three of them; which Creek being out of any Degree of the Stream, there was no Ice at its Bottom, but the Ice on the Top was much thicker, than that on the Surface of the Water in the adjoining Bay, which were only parted by a narrow Neck of Land about two Yards wide.

OBSERVATION XIII.

1. THAT the Warmth of the Earth at ſome Depth under Ground, has an Influence in promoting a Thaw, as well as the Change of the Weather from a freezing to a thawing State is manifeſt from this Obſervation, *viz.* *Nov.* 29, 1731, a little Snow having fallen in the Night, it was by eleven the next Morning, moſtly melted away on the Surface of the Earth, except in ſeveral Places in *Buſhy-Park*, where there were Drains dug and covered with Earth, where the Snow continued to lay, whether thoſe Drains were full of Water or dry, as alſo where

where Elm Pipes lay under Ground; a plain Proof that theſe Drains intercepted the Warmth of the Earth, from aſcending from greater Depths below them; for the Snow lay where the Drain had more than four Feet depth of Earth over it. It continued alſo to lay on Thatch, Tiles and the Tops of Walls.

FINIS.

ERRATA.

PAge 45. Line 9. read *bears*. P. 52. l. 22. r. 0. 231. l. 27. r. 0. 227. P. 53. r. $\frac{1}{72}$. r. 0. 0177. l. 4. r. 0. 00024, l. 7. r. 0. 41. l. 9. r. 0. 00308. l. 10. r. 0. 0000076. l. 15. r. 0. 00000173. l. 17. r. 0. 0000124. l. 18. r. for 0. 00001. r. $\frac{1}{25}$. P. 54. l. 9. r. 0. 231 0. 227. l. 10. r. 0. 41. P. 57. l. 8. r. 0. 000617. l. 9. r. 0. 00193. r. 0. 000000297. l 13. r. 0. 0000237. l. 14. r. 0. 504. l. 19. r. 0 504. r. 0. 496. P. 65. l. 4. r. VIII, Numb. 9, P. 71. l. 9. for *there*, r. p. 57. l. 16. r. 556399. 8. l. 25. r. 556399. 8. l. 26. r. 4940830. P. 72. l. 5. r. 9881660. l. 7. r. *Numb.* 7. l. 9. r. 354180. P. 88. l. 26. r. *Numb.* 1. 7. P. 93. l. 1. r. 4. P. 102. l. 20. r. 54. P. 143. l. 7. r. 3. P. 221. l. 26. add *Tartarine Concretions*. P. 255. l. 1. r. 255. P. 256. running Title, r. *The Appendix*. P. 259. r. *two*. P. 319. r. for.

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